The Angle Orthodontist

April • 1958

Published Quarterly by
The Edward H. Angle Society of Orthodontia

Published Quarterly by the
EDWARD H. ANGLE SOCIETY OF ORTHODONTIA
Zuelke Building, Appleton, Wisconsin
Subscription Rates: $6.00 per year, Canada $6.25,
other foreign countries $6.50.

☆ ☆ ☆

Copyright, 1946, by the Edward H. Angle Society of Orthodontia

The Angle Orthodontist

April 1958

Volume 28, Number 2

Contents

61 • STATISTICS — LIGHTHOUSE OR LORELEI?
Raymond C. Thurow, D.D.S., M.S.

79 • THE ROLE OF THE SOFT PALATE IN CLEFT PALATE SPEECH
A. J. Wildman, D.D.S., M.S.D.

87 • HEREDITARY FACTORS IN TOOTH DIMENSIONS, A STUDY OF THE ANTERIOR TEETH OF TWINS
Sidney L. Horowitz, D.D.S.
Richard H. Osborne, Ph.D.
Frances V. DeGeorge, M.A.

94 • EARLY TREATMENT
Henry A. Sturman, D.D.S., M.S.

108 • CLINICAL OBSERVATIONS OF CASES FIVE YEARS OUT OF TREATMENT
Harold M. Stackler, D.D.S., M.S.D.

Statistics - Lighthouse or Lorelei?

RAYMOND C. THUROW, D.D.S., M.S.

Madison, Wisconsin

Orthodontics has had its share of new theories and technics down through the years, but there has never been anything quite like statistics. Here we find ourselves entangled in something completely foreign to our training and experience and, as a result, it is often misunderstood and even more often mistrusted.

What can we do about this constant statistical bombardment? We can't just ignore it and hope that it will go away. That will probably never happen and, if it did, we would certainly be the losers in the end. The only solution which I can see is to learn to live more comfortably with these statistics, and that is what I plan to discuss at this time — *learning to live with statistics.* This will very definitely not be a short course in statistics — far from it. As a matter of fact, I see no need for every orthodontist to become a statistician. But I feel very strongly that he should learn to become a critic of statistics, just as he is a critic of the many other things which he encounters in every-day living. Take a steak, for instance. I doubt if very many of us could breed a strain of beef cattle, select a choice animal, feed it to maturity, butcher it, and cut and cook the steak. But when we are served a steak, how many of us hesitate to pass judgment on its quality? The same holds true for our automobiles, television sets, and even such relatively simple things as the instruments and materials of our profession. In all of these situations we form our judgments as consumers, basing them on ability to fill our needs — regardless of our ability to design or produce the products themselves.

Our position should be no different with statistics. We are the consumers and in our own interest we must evaluate what we are being sold. The mere fact that we cannot develop and apply all of the mathematical manipulations involved should not deter us at all. Would our patients hesitate to tell us that their teeth are still "crooked" merely because they couldn't correct the situation themselves? Actually you, as an orthodontist, are in a much better position than you may have realized when it comes to evaluating orthodontic statistics. After all, you produce the raw material for the statistician and then use his final product. He is merely the man in the middle. If you know what goes into his process and what comes out, you don't have to know much about the details in between to form an opinion of their effectiveness.

The statistician, on the other hand, is at a distinct disadvantage. He has to take the data which we give him, often without a clear understanding of the problem which concerns us. Then he must select and apply a statistical technic and finally present his results in a form which will be understandable and useful to us. Of course, in many cases the orthodontist may also be the statistician, but this still leaves the same problems to be solved. It just gives the task of solving them to a com-

Based on material presented before the following meetings: Reunion meeting of graduates of the Orthodontia Department, University of Illinois March, 1956, Biennial Meeting Edward H. Angle Society, Washington D. C. October, 1957.

parative amateur. The statistical process is basically the same as nearly all other phases of productive human endeavor. Raw material is gathered and subjected to a process which converts it into a usable product. An error of judgment or action in any of the stages can adversely affect the results, so in our evaluation they should all be considered.

The Sample and the Data

Statistics is by its very nature a series of compromises and they begin immediately in the collection of data. For instance, we cannot measure all of the Class II malocclusions in the world, so we compromise and take just those from our own practice between certain dates. This is our sample. It will probably be assumed to be the same as any other group of Class II cases, but there may be significant differences depending on racial background, our classification of malocclusion, etc.

Next, we collect data intended to describe this sample in terms which can be subjected to mathematical analysis. Here again we make a series of assumptions about just what our figures are describing and how well they describe it. I am not going into detail on these problems, because they vary too widely in different situations. Whole books have been written on sampling, but merely evaluating the assumptions mentioned above will provide a good foundation.

The Statistical Technic

With the data in hand, the next problem is deciding how to process it. There are many statistical technics available and most of them were developed originally for specific situations. To my knowledge, however, none of them were developed for orthodontics. They all have their weak points as well as their advantages and it is the responsibility of the user to know them both. The problem is that any of these mathematical procedures can be applied to *any* set of figures which are fed into them, relevant or not, and the results will look just as imposing in either case. Actually the results may be invaluable, useful, meaningless, or downright misleading, but they won't be labelled for us.

A cotton picker may be an excellent device for picking cotton, but if it is run through a cornfield the results could be disastrous. Yet in statistics more than one cotton picker has been run merrily through a cornfield — and many people have been sold the crop which it tossed into the wagon.

Statistical technics are not as clearly identified as cotton pickers, but the facts are really not secret. Authors usually refer rather glibly to the statistical technic which was used, as though everyone would understand, and they are probably right in not going into details. But most elementary — and I stress the word elementary — texts in statistics will state in clear, understandable English just what can and cannot be done with each of the commonly used statistical methods. There is no need to go into the mathematical details of how it operates to find out what it will do.

Interpretation

When our information emerges from this statistical mill, we have just one more hurdle left — we have to interpret and apply the findings. This is the most important phase of all in some respects, since it is these findings which lead us to the conclusions which were the real object of all this effort.

It is really true that statistics do not lie, in spite of many implications to the contrary; the real difficulty is that they speak a foreign language which can sound deceptively like our native tongue. Our problem is to know when

we need a translation and then translate into something which can be understood correctly. All of the previous stages enter into this final statement of the findings. The statistical result is nothing more than an innocent little figure which tells in all honesty what happens when the originally selected sample is measured in the manner in which the data were collected and the data subjected to the mathematical procedures used. If it is read and evaluated in this light, with all of the assumptions made along the way thrown in, it will present a true picture. Whether it is a picture of what we wanted, from the point of view which we wanted, is for us to decide.

Enough for the generalizations. They have been kept as brief as possible to give the necessary background for the illustrations which follow. The points which I will discuss in the balance of this paper could be easily covered in the usual abstract mathematical terms, but I feel that this would only duplicate what can already be found in almost any statistical text. Instead, I plan to take specific illustrations from the orthodontic literature and point out where I feel that statistics have been misapplied or misinterpreted.

The selection of these illustrations was actually quite unscientific, since they were merely selected from articles which I happened to notice in the course of my reading. They include only the work of authors for whom I have a very high regard, which is one of the reasons why I read their work in the first place. The comments which I will make here in no way lessen this regard. I must emphasize, however, that since this is a critical review I am concentrating only on errors which I have found. These works are all valuable contributions to our profession and contain much important material which will not be mentioned here.

"Investigations on the Orbital Plane"

Statistics came into orthodontics even before cephalometrics, but my first illustration is a harbinger of things to come. This article by Broadbent[1] was a refutation of Simon's contention that the orbital plane represented a guide to cuspid position in the determination of the occlusion. It was based on a group of skulls from the collection at Western Reserve University, all Caucasians with normal occlusion. Using the craniostat, equipped with a special pointer for this project, Broadbent accurately measured the point at which the orbital plane passed through the dentition. The graph in Figure I shows his results, expressed in terms of the frequency with which the orbital plane passed through the mesial and distal half of each tooth.

This illustration (Fig. 1) is a remarkably clear presentation of the data and can leave little doubt that the orbital plane is indeed no reliable guide to cuspid position. He has made his point clearly and decisively, but he then goes on to use more statistics. These are shown in Figure 2. The upper series of bar graphs show the mean distance of each tooth or anatomic point from the orbital plane, while the lower series show the coefficient of variability for these points. Note particularly that while the cuspid shows the smallest mean distance in the upper series, it shows the greatest variability as expressed by this criterion in the lower series. Just what is this coefficient of variability? What does it tell? Should it have been used here?

Suppose that you wanted to compare the prices of hogs and corn, to see which varied the most. With hogs about twenty dollars per hundredweight and corn about two dollars per bushel, com-

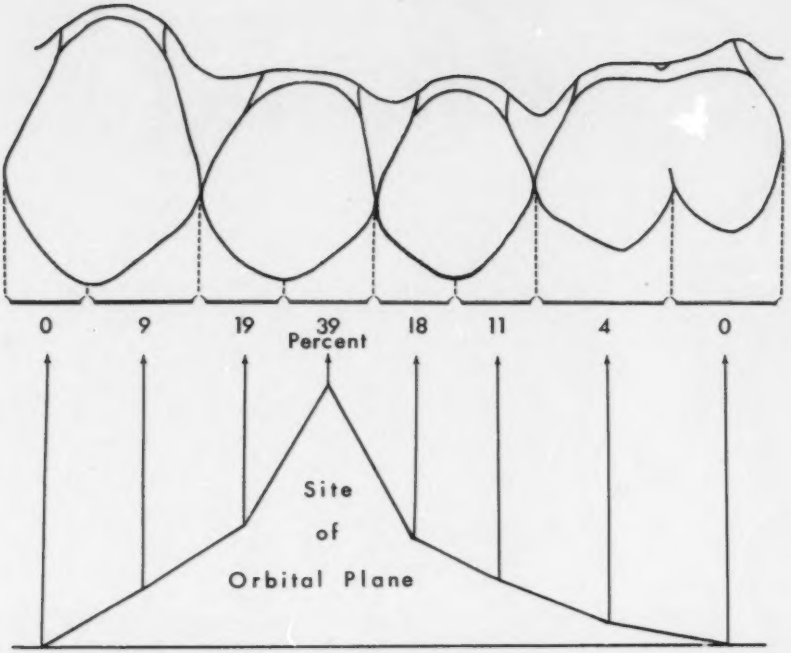

Fig. 1 Broadbent's illustration showing the frequency with which the orbital plane was found to pass through the mesial and distal half of each tooth. Note that it was most frequently found to pass through the distal of the upper first bicuspid.

parison in terms of dollars alone would be misleading. After all, one dollar of variation on a twenty dollar item is only five per cent, while one dollar of variation on a two dollar item is fifty per cent. Here the statistician does essentially what I have just done — he expresses the variations in terms which bring out these percentages. He uses a more refined technic, but the basic principle is the same. He uses the standard deviation to represent the variation, to reduce the effect of extreme cases, and then he divides it by the mean of all the measurements. The result is the coefficient of variability.

This is all very well as long as you are measuring from a true zero point, such as counting dollars or people; or even measuring the standing height of people as shown on the lower bar in Broadbent's illustration. The trouble arises when you introduce an arbitrary zero, since this also gives you an arbitrary denominator. In this particular example the denominator is not only arbitrary, but it is the very base which the author is trying to prove is unreliable. What happens is that the closer the zero point moves toward the point being measured, the higher the figure for the coefficient of variability becomes by the simple expedient of dividing by a smaller number. If the standing height used were measured from a point three feet above the floor

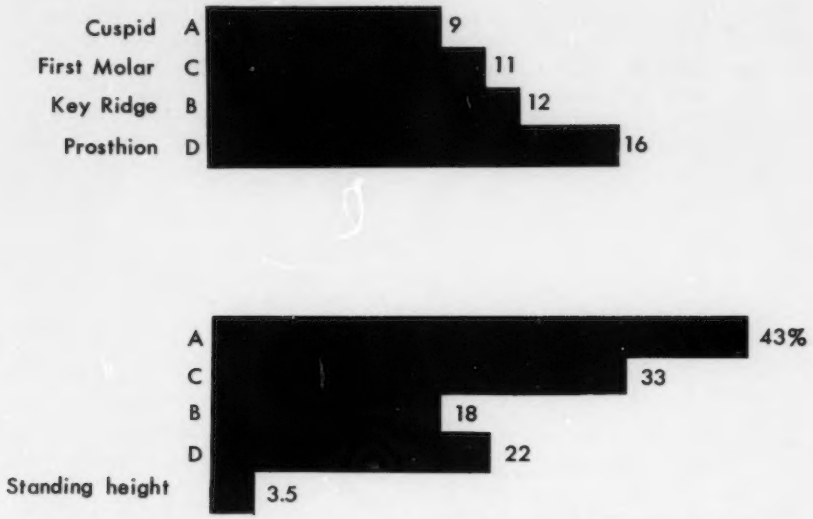

Fig. 2 Broadbent's graphs showing mean distances from the orbital plane (above) and the coefficients of variability (below) of these distances. The coefficient of variability for standing height was included for comparison. See text.

instead of from the floor, the coefficient for a group averaging six feet tall would be doubled, since the same variation would be divided by three feet instead of six.

Going back to the orbital plane, since the cuspid was closest, it had the smallest denominator; hence, the highest coefficient of variability. If the zero point for these measurements were moved out nearly six feet, as is the case for standing height, the coefficient would have dropped much lower than that for standing height.

With this information on how to get the coefficient of variability and the mean distances from the orbital plane, we can now backtrack and see what the actual standard deviations were for these teeth. They are found to be 3.9 mm. for the cuspid and 3.6mm. for the molar. This insignificant difference was magnified by dividing those figures by the distances from the orbital plane. It happened to work out nicely in this case, but it is fortunate that Simon was as close to being correct as he was. The cuspid still averaged two mm. closer to the orbital plane than the molar but, if the molar had been closer, the whole picture would have been reversed. Then the cuspid would have appeared more stable. Had Simon been completely correct, and the cuspid coincided perfectly with the orbital plane, the denominator for the cuspid in these calculations would have become *zero*. Then the smallest measurable deviation from the orbital plane would have made the coefficient of variability equal to *infinity*! Since the mean position for the orbital plane was actually in the bicuspid area, the figures for these teeth undoubtedly did reach great heights.

The general conclusion to be drawn here is that we must not forget that statistics are inanimate figures, completely lacking in discretion. If they are used improperly, the results will look no different from valid results; the

burden of proper application and interpretation is ours. The specific point in this case is that the coefficient of variability is used to evaluate variability in relation to the total measurement. When the total measurement can be changed merely by changing the base point from which it is measured, the concept becomes completely meaningless and is not applicable.

"Etiology of Class II Malocclusion"

To see what choice of samples and interpretation of findings can do, we now go on to a series of theses from the University of Illinois which have led to some rather startling conclusions. These are the theses written in 1939 and 1940 by Adams,[2] Elman,[3] and Baldridge,[4] which were discussed as a triad by Brodie[5] at the 1948 University of Illinois reunion meeting.

I will discuss them in order beginning with Adams. He measured the mandible on cephalometric films of patients with the three Angle classes of malocclusion. One of his most significant findings was that the mandibles in Class I and Class II malocclusion showed no essential differences in either gonial angle or absolute dimensions. In thirty-two Class III mandibles he found a mean gonial angle six degrees larger than the means for the first two classes. From this difference, his conclusion in the original thesis stated that "The gonial angle is always greater", although an examination of the figures shows that about ten percent of the Class I and Class II cases had gonial angles *greater* than the Class III mean. Conversely, many of his Class III cases had gonial angles *smaller* than the Class I and II means. In other words, there is a large amount of overlapping in these groups which would not be anticipated by merely reading the conclusions.

In Elman's thesis, he measured the positions of the lower molars and plotted them on a graph as shown in Figure 3. The distal contact point of the lower first molar was located and its distances from the lines representing posterior and inferior borders were measured. These dimensions were then plotted on the graph. The scattergram in the illustration represents his control group. On this type of scattergram, he then measured the angles formed with the base line by lines drawn from the origin to the individual points. One such angle is shown on the illustration by the shaded angle between the dotted lines. In essence, this graph converts all gonial angles to a standardized 90° before the points are plotted and the angles measured, so that the angles which he uses are geometric constructions, *not* angles which can be read directly from the cephalometric radiograph. This is a legitimate approach, however, which might be considered in other situations where variables could be standardized in this manner.

Elman found mean angles of about 34.5° for both the control and Class II groups, with standard deviations of .94° for the controls and 1.22° for the Class II group. Two thirds of the controls and half of the Class II cases fell between 33° and 36°. These are certainly narrow limits of variation; so narrow, in fact, that they inspired Brodie to make this statement at the first Illinois reunion meeting: "I think you will all agree that Dr. Elman's findings were striking, to put the matter very mildly. When one mentions standard deviations of .94 and 1.22, the narrowness of variation suggests physical and not biological studies."[5]

But just how remarkable is this? A standard deviation by itself means very little. It must be seen in the light of the units of measurement which are being used and the range of variation which

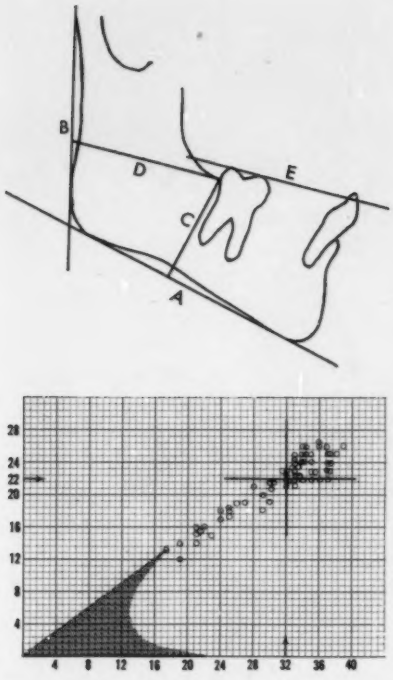

Fig. 3 The position of the lower molar in the mandible as measured by Elman. Dimensions D and C shown on the mandible were recorded on the graph as a point for each case. The graph shows the result for the control group. The angle shown by the shaded area was then measured for each point and used as the indicator of molar position.

might be possible. If Elman had made his measurements in hundredths of a degree instead of in degrees, the standard deviations would have been 94 and 122. Even more important in this situation is the possible range of variation. When we think of degrees, we usually think in terms of 360 of them arranged in a circle. If we have them so arranged on a carnival wheel and spin the pointer, and it stops within one degree of a certain number on two-thirds of the spins, we would no doubt suspect that the wheel was rigged. And Elman's wheel was rigged — not by Elman, but by anatomy. If we put a mandible on the wheel, as he did, and decide to count only the times that the pointer stops on a tooth, the odds will be changed considerably. Actually, it takes only twelves degrees to sweep from the distal of the first molar to the central incisor, while six degrees cover the first molar. Three degrees will cover one molar cusp, the difference between a Class I and a Class II relationship, and half of Elman's Class II group and a third of his controls varied *more* than this.

Looking at it in still another way, Figure 4 shows the range of molar position which he found for Class II cases. The shaded area represents the area in which he found the distal contact point of the lower first molar in the various cases in his group; molars have been added in the extreme positions to show the range more clearly. This is the same range which, by the original criterion, was described as "strikingly narrow" in variation.

In Baldridge's thesis, he measured the angle N-S-$\underline{6}$, using it as an indicator of upper molar position. Here again, no essential difference was found between Class I and Class II cases, but the standard deviation was about three degrees. This standard deviation, though it sounds small, means that one third of the cases varied more than two full molar cusps in mesio-distal position.

That is the essence of the findings. Now, what do they mean?

In Brodie's report on research at Illinois, he stated after Elman's work that "We considered the point proven that the relation of teeth to mandible was identical in Class I and Class II."[5] These are strong words. The dictionary defines "identical" as the strictest term for entire and absolute agreement, such

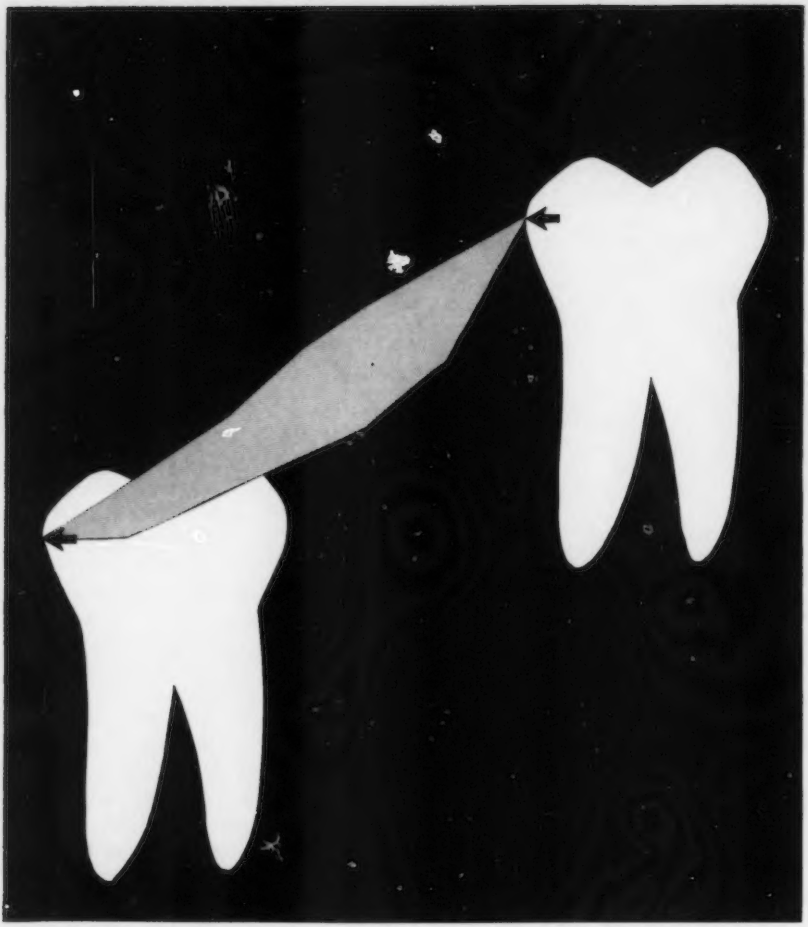

Fig. 4 The range of molar position within the mandible, as found by Elman. The shaded area shows the range of position for the distal contact point, which is indicated by the arrows on the two teeth shown. These two teeth are in the extreme supra-anterior and infra-posterior positions of the range, and are shown to scale.

as "no two leaves are identical".

Following Baldridge's findings, we heard that "The remarkable stability of the molar teeth was demonstrated",[5] and "Apparently the upper first molar was not the unreliable will-o-the-wisp that it had been held to be".[5]

This is the interpretation which has been placed on this work, and it is this interpretation which has been used as a springboard to even broader conclusions which I would like to discuss next.

Interpretation of these results has followed a pattern of reasoning which can be condensed to these three steps:

1. The positions of the molars in their respective arches have been shown by Elman and Baldridge to be the same for both Class I and Class II malocclusion.
2. The lower molar is actually distal to the upper in Class II, therefore the mandible must also be posterior.
3. Adams has shown the mandibles to be the same size, therefore the condyles and the glenoid fossae of the temporal bones must also be posterior.

There was a hint at this conclusion in the very first of these theses, that of Adams, when he said: "Since the Class II mandible shows little or no difference from the normal pattern, it seems entirely possible that this type of malocclusion may depend for its inception upon a growth disturbance in the cranial base. One can easily visualize, for instance, an arrest in the temporal area which would act as a deterrent to the normal forward movement of the mandible with growth."[2] Of course this is easy to visualize, and it makes interesting speculation, but it is only one of *several* possible causes for Class II. It is also only one of several possible interpretations for this research.

Another Look

At this point I would like to go back to the beginning and take a second look at this entire situation.

The first step in all of these studies was the selection of the original samples for the data. All of these men selected their cases without regard to the severity of the deformity. This may be democratic, but the effects on the results cannot be overlooked. In random groups such as these, the different classes must almost certainly be more similar than if only classical malocclusions were used. In each of these classes will be many cases which tend strongly toward another class — the "borderline" cases. These cases will inevitably bring the means for the groups closer together.

Choice of control groups raises another question. The controls were all Class I malocclusions except Elman's, which I will discuss separately. These were malocclusions, *not normals.* In the practice of orthodontics, what is the most common characteristic of Class I malocclusions? Without running a new survey, I would say that it is crowding. How many times do we see a Class II case with a crowded upper arch and a perfect lower arch? If the lowers were crowded like the uppers, the occlusion might well be Class I. In comparing such cases, would it be surprising to find the upper molars in similar positions? Or would it be surprising to find the mandible in the uncrowded Class II larger than in the crowded Class I case? Cases such as these can help to bring the means for the control and Class I groups closer together.

Dr. Elman's controls were even more interesting. They were about five years younger than his Class II group, one third of them actually under six years of age, with the molars which he was measuring still unerupted! Furthermore, to use his own description, "The greatest majority had Class I molar relation, a few Class II, and two or three cases were Class III." This is the group which was treated as a normal control. They may have been normal children in a more general sense of the word, but he was studying occlusion, and controls should be selected accordingly.

Now for the findings; just how much stability do these studies actually show? Yes, the lower molars do stay remarkably close to Elman's 34.5° line, but what does this have to do with oc-

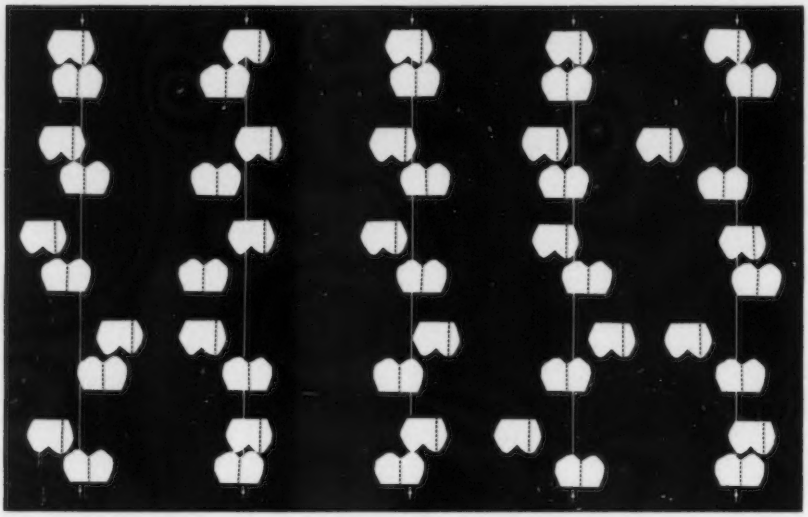

Fig. 5 Upper and lower first molars from twenty-five normal occlusions, rearranged in the occlusal relationships which they would assume if the measurements of Baldridge and Elman were the sole determinants of their positions.

clusion? Staying on this line, one millimeter of change in vertical position will produce two millimeters of horizontal change. How can we possibly consider this to be horizontal stability? Yet it is horizontal position which determines occlusal relation.

Baldridge's cases showed equally wide variation, with one third of the cases varying over a range wider than a molar. What kind of stability is this, when the difference which concerns us, the difference between Class I and Clas II, is only half as great?

Adams' mandibles showed similar means for Class I and Class II, but there was wide overlapping in both directions.

In view of all this, we have made some additional measurements on a series of twenty-five cases with normal occlusions. If the similarity between the means for these measurements in the different classes is to have any meaning at all in the determination of occlusion, then there should be some correlation on an individual basis. In other words, the case with upper molars distal to the mean as measured by Baldridge should also have the lower molars distal as measured by Elman, etc.

To test this possibility, we measured these normals as Baldridge and Elman had done, and calculated the mean values. The mean for N-S-$_6$ was 64°, compared to 68° for Baldridge's group. The mean for the lowers was essentially the same as Elman's. The amount and direction by which the first molar teeth in each case deviated from the mean was then converted to millimeters and recorded. A vertical line was drawn to represent the mean, the outlines of the crowns of upper and lower first molars were placed in position for each case according to the measurements. The results are shown in Figure 5. In normal occlusal relationship, the dotted lines on the two teeth in each pair would

be in alignment, and if they were also in the mean position for the group, they would line up with the white background line as well. How fortunate it is for humanity, and for orthodontics, that other factors have entered into the determination of the occlusal relationship!

So what does all this prove? Statistics never *prove anthing;* they merely present information. The burden of interpretation is ours, as authors and readers. Any conclusion which fits the facts must be considered a possibility until more facts become available. You, as reader, have both a right and a responsibility to consider all of these possibilities.

In this particular situation, Baldridge summed up the possible causes of Class II relationship as follows:

1. Overdeveloped maxilla.
2. Maxillary teeth forward.
3. Underdeveloped mandible.
4. Mandible located distally.

The fifth and sixth possibilities, that the lower teeth might be posterior on the mandible or that the maxilla might be positioned anteriorly, are apparently ruled out in advance.

Through all of this work and its interpretation for us, has run the implied assumption that only one of these possibilities can be true; that all we need to do is to find out which one it is. Yet I know of no evidence for such an assumption. On the contrary, our education and experience are constantly impressing us with the wide variety of individual variations.

What has happened here is a following of the mean to the exclusion of all other pertinent information. The means have been compared and conclusions drawn from them with a total disregard for the majority of cases which varied widely from those means and overlapped considerably into the different classes. On the basis of means, which are a method of obliterating variation, the molars have been held to be remarkably stable. But another look at the total information derived in these very same studies shows those molars to be just about as wispy as they can be and still remain between the second premolars and the second molars. Certainly no molar stability of any orthodontic significance has been demonstrated.

These variations also suggest that, far from eliminating all but one of the possible explanations of Class II etiology, we should consider all four of them and add numbers five and six to the list. There is nothing shown here which should eliminate mesial positioning of the upper teeth or maxilla, distal positioning of the lower teeth or mandible, overdevelopment of the maxilla, or underdevelopment of the mandible.

A Class II relationship looks more and more like a symptom rather than a specific disease. It takes us about as far toward a diagnosis as the presence of fever takes the physician toward his. Grouping otherwise unrelated cases into such a superficial classification and then calculating means only serves to obliterate the individual differences which are the real bases of the problems.

Cephalometrics, statistics, means and norms are all very useful devices in orthodontics, just as they are in many other fields. But they must be kept in proper perspective. Each gives another point of view which can tell us much about our problems, but we cannot afford to be carried away to the point where we forget that there may be other vantage points which could give us still more views. Unfortunately, we have no way of knowing when we have finally attained the true and total view

— another might turn up tomorrow. The important point is to continue looking.

"A Cephalometric Evaluation of Fifty-Seven Consecutive Cases Treated by Dr. Charles H. Tweed"

Up to this point, the illustrations have been more or less ancient history. For this last analysis we would like to take up something a little more recent. This paper, by Stoner and co-workers,[6] is an attempt to explain Tweed's success at attaining his results by an evaluation of his treated cases. The authors are particularly interested in the Frankfort-Mandibular Incisor Angle (FMIA), and its relation to the soft tissue changes produced.

In the introduction, Stoner refers to Wylie's interpretations of the results in twenty-nine of Tweed's cases,[7] in which he (Wylie) questioned the validity of the FMIA. Here he says that "Wylie states that Tweed is more successful in obtaining the kind of facial change he advocates than he is in explaining his own successes. The purpose of this paper is not to take issue with Wylie, but to render an impartial and scientific analysis of the changes brought about in these treated cases." This is a worth-while goal, but have they attained it?

First they take up the problem of superimposing tracings to evaluate the results of treatment. Their method of evaluation is to superimpose the tracings on the S-N line, registering at nasion, as shown in their first two illustrations.

This method reduces the effects of forward growth, although it does not "neutralize the dimensional changes of the skeletal structures in areas not affected by mechanics" as they optimistically state at one point. Unfortunately, the effects of growth *cannot* be neutralized by any single superimposition. Any change between the point of superimposition and the point being observed can be the result of either growth or treatment, and both possibilities must *always* be considered. These effects become apparent in many places throughout the paper.

In this particular instance, any area with antero-posterior growth different from that on the S-N line will show a change in the superimposition. It will show a retrusion if growth was less and a protrusion if growth was greater. Vertical differences will show additional effects, even in the horizontal direction.

The first changes studied (page 72) were those of points A, B and P. Antero-posterior changes were measured parallel to the Frankfort plane and tabulated. The results are shown in Table I.

TABLE I

Point	Mean	Range	Standard Deviation
A	—1.8	—7.0 to +2.0	1.86
B	+ .08	—5.0 to +5.5	2.16
P	+ .57	—6.0 to +8.5	2.59

Point B was found to vary over a range of more than 10 mm., while P ranged over more than 14 mm. The standard deviations show that about two thirds of the cases fell within a little more than 2 mm. of the mean, or over a total range of 4 or 5 mm. Now, look at the means. Those for both B and P are very small, but here is the conclusion which was drawn from them; "For the fifty-seven cases as a whole, point B moved forward only .08 mm. as represented by the mean. Point P moved forward approximately .6 mm. or more than six times farther anterior than point B."

A comparison of means such as these leads to the same type of fallacy as the coefficient of variability which was discussed earlier. If the measurements are

made from a true zero, in only one direction (positive or negative), it would be possible to compare means in the form of such a ratio and say that one group averages six times larger than the other. But look at the situation in this case. Table I shows about the same variation on both sides of zero, with the means actually very close to zero. If one of the means had shifted slightly, so that it was negative, this type of comparison would require us to say that P moved forward minus six times as much as B. Or if the mean for B was *zero* (it really was zero, if the .08 had been rigorously reduced to the proper significant figure), then P would have shown *infinitely* more movement than B. Even using the figure of .08 an actually insignificant difference was inflated to six hundred per cent.

On the basis of these same small means, they then go on to say, "The forward movements of these two points are surprisingly small, but it is noted that P moves forward much more than B, which tends to confirm our assumption that point B can be affected or moved slightly posteriorly when the lower incisors are moved lingually, or that appositional growth occurs on the chin point." In one case point P moved forward 8.5 mm. (using their superimposition), which can hardly be called small. Of course, some moved lingually to bring down the average, but with ranges of 10.5 and 14.5 mm., I would certainly not consider the changes to be small. On such a plus or minus basis (coming and going), the mean motion of the race cars at the Indianapolis Speedway is also zero, but we could hardly call their movements small.

As for the assumption referred to in the last part of the quotation (that B can be moved distally), I can see no confirmation of it in these figures. With averages near zero, and ranges in both directions almost equal, I would consider the most liberal interpretation to be that the figures do not refute the assumption.

Point A was measured in the same manner as B and P, with the results also included in Table I. Although the mean change of —1.8 mm. is not tremendous, it is certainly worth noting. And as the authors point out, a look at the ranges as well as the means shows many interesting changes. In their reflection on whether point A can be moved posteriorly or whether the mandible can come forward, I find this particular sentence most interesting: "In fact, at the conclusion of treatment, in twelve cases points A, B, and P were all posterior to their original positions when related to SN at N." I can see Dr. Tweed wanting to move point A posteriorly, and I can see him wanting to move point B posteriorly, but do the authors mean to imply that he intentionally moved the chin *posteriorly* on one fourth of his Class II cases?

If treatment is to be credited with all the other changes, it should be credited with these as well, although I am inclined to attribute many of the changes to growth. The only explanations which I can think of for this posterior movement of the chin in the superimposed tracings are that nasion grew forward faster and left the chin behind, that the mandible was displaced distally, that it was tipped distally in bite opening or growth, or that it was reduced in size. My own guess, and it is only a guess, is that most of these changes are due to the first of these possibilities, a differential in growth. Distal displacement also should be considered. As for bite opening, the authors' statement that the Frankfort-mandibular angle change was negligible in most cases would seem to rule it out as a major factor, but their figures were not published and the term "negligible" can mean different things to

different people. The last possibility, an actual reduction in mandibular size, would not be acceptable at the present time, and would probably be eliminated by linear measurements.

Going on from here, they ask (page 74) "what is the likelihood of these things happening in any given case that we may have to analyze before starting treatment? Here statistics offer the best method of predicting possibilities." They now go into the coefficient of correlation, a figure which we encounter all too frequently. In this work it was used to evaluate the relationship between the original discrepancy between A and B and the reduction which was attained in treatment. However, before I go into their figures, I would like to elaborate a little on the subject of coefficient of correlation.

Here again, it is not necessary to be able to calculate it in order to have some usable idea of what it represents (and does not represent). If there is some relationship between two measurements, so that a change in one is followed with predictable consistency by a change in the other, we may say that there is a correlation. The coefficient of correlation is supposed to tell us just how predictably consistent this relationship actually is.

Perfect correlations are found regularly in fields such as physics. For instance, if we measure the tension on a a spring and also measure its length, plotting the two on a graph will soon make it possible for us to tell what one will be by measuring only the other. As long as we stay within the elastic limit of the spring, our predictions of length from tension and vice versa will be perfect, and so we have a perfect correlation. In biology things don't usually work out so precisely. Here we are usually scratching around looking for just a little hint that perhaps cases with big A-B differences change more than cases with little A-B differences.

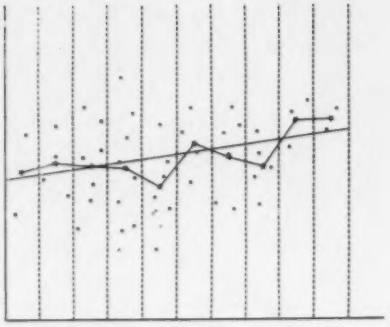

Fig. 6

The basis of the coefficient of correlation, which tells us just how much of a hint there is, is the scattergram which plots one factor against another. One such scattergram is shown in Figure 3, although Elman used it for less sophisticated purposes. Another is shown in Figure 6. This is a purely hypothetical illustration, which will be used merely to assist in this explanation.

When we have plotted our scattergram, we take leave of precise figures and concrete facts and venture forth into a world of erudite guesses and compromises which will eventually lead to a decimal figure with a value between -1.0 and $+1.0$. It is this figure which is known as the coefficient of correlation. The details of how it is found are too involved for this brief exposition, but a bird's eye view of the process can be a great help in looking one of these little figures squarely in the eye.

Briefly, the coefficient is obtained by first dividing the scattergram into vertical columns as shown in Figure 6. The width of these columns is decided somewhat arbitrarily. From this stage on, each point on the graph is treated as though it were in the center of its particular column. The mean height of all the points in each column is then calculated and marked on the graph.

These means are indicated by stars in Figure 6.

When the means are connected, they usually produce a rather irregular line such as the one in the illustration, so the next step is to straighten it out. This is a mathematical procedure which establishes the straight line which comes *closest* to the original crooked one. It may not be very close, but we can rest assured that it is as close as we can get with a straight line, and if we want a coefficient of correlation, the line *must* be straight. Once we have this line, which is called a regression line, we are well on our way.

The next step is to draw a series of horizontal rows on the graph similar to our vertical columns, and go through the whole procedure all over again to establish a regression line for our series of horizontal means.

What the coefficient of correlation tells us is how close these two regression lines come to coinciding. If all of our original points were on a straight line, the two regression lines will be on the same straight line, identical with each other. In this case the coefficient will be one, the highest attainable. On the other hand, if the points are scattered in a circle, the line for the columns will be horizontal and the line for the rows will be vertical. This is as different as they can get, and the coefficient in this case will be zero.

With a coefficient of one, if you know the value for one measurement, you can predict the other precisely. With a coefficient of zero, knowing one of the measurements doesn't even give you a clue to what the other might be. Of course, most coefficients will lie somewhere between these extremes, so we immediately wonder what it means if it is .20 or .50 or some other figure. Here there can be no exact answer, because what is significant in one situation will be insignificant in another. There is a considerable subjective factor in interpreting these figures once you have them. Actually, they are most useful among themselves, when you can say with confidence that one coefficient is larger than another, etc. For such a comparison to be valid, the coefficients must be based on *very* similar data — such as test scores for different groups of the same age.

Actually, the original scattergram is frequently more meaningful than the coefficient, in spite of the fact that it is easier to obtain. The safest procedure, especially when many readers will not be professional statisticians, is to present the scattergram with the coefficient, as was done by Wylie in his evaluation of Tweed's treatment.[7]

We cannot leave the coefficient of correlation without mentioning one very common misapplication — using it as an indication of a cause and effect relation. It gives absolutely *no* such indication. Many completely unrelated items can vary in a similar pattern and so give a positive correlation. The current increase in juvenile delinquency is being accompanied by an increase in classrooms and in orthodontic treatment, but no one would seriously use such a positive correlation as an explanation of cause of juvenile delinquency or malocclusion. If cause and effect exist, there should ordinarily be a good correlation, but the converse definitely cannot be assumed without other supporting evidence.

Getting back to the problem at hand, the authors found a correlation of .48 between the original difference between A and B and the amount of reduction which was attained by Tweed in his treatment. This would be generally accepted as a significant correlation, but it is really not surprising. After all, the more the discrepancy is at the be-

ginning, the more likely it is that the incisor roots are contributing to the location of these points, making them more easily changed. Even more important, the greater the discrepancy at the beginning, the greater the effort which will be put into reducing it — and the greater the possibilities of reduction. In the case with A directly above B, with no difference, I doubt if there was any great effort made to change it; and how would you go about reducing a zero difference in the first place?

When an attempt was made to correlate changes in the individual points A and B with the original A-B difference, the results were rather disappointing. For A it was .33 and for B it was .18. This is another place where statistics do not tell enough of the story. The authors had noted earlier that in ten cases with unfavorable A-B relationship point B actually moved distally. Considering Tweed's technique for anchorage preparation, this is certainly not surprising, but it can do a lot of damage to the coefficient of correlation. The coefficient in this case just does not evaluate what Tweed tried to do.

The same problem arises on the next page where tooth movement is related to overjet. A significant correlation of .61 was found between the initial overjet problem and the movement of the upper incisor, while the lower correlation was insignificant. Their conclusion is (page 75): "On this basis, Dr. Tweed could be reasonably sure he would have a fairly consistent and considerable movement of the upper incisors in those cases with a large overjet. The response of the lower incisors would be unpredictable." A much more reasonable conclusion could have been reached by looking at the patient, or the casts, or the cephalometric films. I feel that this interpretation sells Dr. Tweed short, through misapplication of the statistics.

Statistics functions primarily to clarify by abbreviation. The initial data is summarized, details are eliminated, and the final results presented as a concentrated essence of the original data. In this case, only two factors were considered—the original overjet and the change in tooth position relative to nasion following treatment. All other details about the diagnosis and treatment of the problem are intentionally eliminated. On the basis of this limited information, the conclusion (page 74) is that, "When Dr. Tweed has a severe case as represented by a poor relation of denture bases or points A and B, he can expect a moderate chance that this poor relationship may be improved in varying degrees during treatment." Why handicap ourselves so? We are all orthodontists, and we have much more valuable guides for making such predictions. The most important of these is what does Dr. Tweed *want* to do? The correlation used here assumes that he wants to do anything which will reduce the overjet — whether it be to move upper incisors posteriorly or to move lower incisors anteriorly. I need not belabor the folly of this assumption.

What the correlation actually says is that we have only a moderate chance of predicting the *degree* of change in AB on the basis of the original difference. This says nothing about whether a case with a high AB discrepancy has a chance of being improved. Even with a correlation of zero, all of the cases can lie in the area of having an AB discrepancy at the start and of having an improvement at the end of treatment. The low correlation could merely mean that some of the severe cases didn't improve as much as some of the milder ones.

So it may be true that a mathematician, given only the figures for overjet and tooth movement, could not predict which way the incisors would move; but an orthodontist with a knowledge of the problem might make a fairly accurate prediction. Certainly Dr. Tweed would be in a position to predict more than a "moderate chance of improvement in varying degrees", or whether incisors would go forward or back.

Moving on now to the conclusion, we again find the Frankfort-Mandibular Incisor Angle which was the initial reason for this evaluation. The authors say (page 95), "Facts are facts, and cannot be altered. Dr. Tweed, using his present methods directed along FMIA control, is producing startling facial changes. Through the great maze of measurements and statistical data we have not yet explained why it works." This is exactly the position which Wylie said Tweed was in, and which this evaluation was to clarify. "However," they state, "we believe that the explanation is simple when all the facts are considered."

Here the impartial scientific analysis is forgotten as they conclude the article with their own subjective explanation. This explanation is basically that distal movement of teeth allows distal movement of bone and soft tissue, thus altering the profile. Essentially, it is an amplification of the statement by Tweed in his article on FMIA[8], "Analyzing the results of this project, the implications are that the orthodontist, as a general rule, should endeavor to move the denture posteriorly as far as possible and rarely resort to the intentional mesial displacement of teeth in the buccal segments during treatment."

Nothing is said about the 65° which is the basis of FMIA control. Does it really mean anything, or is it merely a rallying point? Perhaps this is its real function; a goal to be achieved if at all possible, and then exceeded if at all possible; a basis for rationalizing comparatively extreme lingual incisor movement. When other criteria might give pause, this one beckons ever onward and upward, so that the very maximum can be attained. Certainly nothing more concrete has been presented here.

Discussion

This paper is an attempt to point out some of the hazards with which we must contend in applying statistics to orthodontics. To illustrate typical situations, several examples of statistical fallacies have been selected from the literature for discussion.

Since this is a discussion of hazards and fallacies, it must necessarily be of a critical nature. This makes it doubly important that we point out one additional hazard at this time. This is the hazard of placing too broad an interpretation on the statements which have been made here.

To the best of my ability, I have said exactly what I mean, no more and no less. The illustrations used are specific cases, and other situations must be evaluated individually on their own merits. This also applies to other parts of the articles quoted, since no attempt has been made to evaluate the complete works in detail. The omission of the parts which were not discussed here means only that they were not selected as illustrations, and it should not be construed as either criticism or endorsement.

In other situations these same authors have used statistics correctly, and other authors have also made similar mistakes. These same statistical methods have also been used both correctly and in-

correctly in other situations. In other words, this is not an attack on the authors or the works which were used for illustration, nor is it an attack on statistical studies in general. It merely shows the steps which I have followed in reading statistical studies to try to answer the question posed in the title of this paper; "Are statistics a lighthouse or a Lorelei?"

You may recall the legend of the Lorelei, the rock on the Rhine. The whistling wind sounded like the voices of maidens, luring the river sailors to steer their boats to destruction on the rocks. Actually, those same sounds could have guided them on a safe course, if they had known their meaning and heeded the warning.

A lighthouse, too, is both a sign of danger and a symbol of safety. It is almost always situated directly on top of a navigational hazard. The vital difference between tragedy and safety is determined not by the lighthouse but by the manner in which it is used. On approaching it, the careful navigator will first identify it and then locate it on his charts. Studying this information will give him the necessary data to locate the safe channel, plot his course, and sail on in safety. An error in any of these steps could be disastrous, but used properly through the years the lighthouse has become the very symbol of safety.

If we follow the same procedure with statistics, identifying them, learning exactly what they have to tell us, and proceeding accordingly, they too can lead us through strange waters to new goals. On the other hand, if we merely try to embrace them blindly on the basis of a first impression, we have no one but ourselves to blame if we pile up on the rocks.

Meanwhile, we must give up the alluring prospect of finding pat answers to our problems in little magic numbers, and face the fact that we are dealing with one of the most complex areas in the body. Within this area we are concerning ourselves with some of the most minute variations which concern anyone in the healing arts. And if this were not enough, we are working with it while it is growing, constantly changing in size and proportion. In this complex of growth and change, physiology and psychology, function and malfunction, all in precarious balance, we can find many small deviations from normal, as well as variations within the normal, which can produce the discrepancies which concern us. Sweeping them all together under a statistical rug can give us only passing comfort.

905 University Ave.

REFERENCES

1. Broadbent, B. Holly: Investigations on the Orbital Plane. *Dental Cosmos* Ixix: 797 Aug. 1927.
2. Adams, J. W.: Cephalometric Studies on the Form of the Human Mandible. Thesis, University of Illinois, 1939.
3. Elman, Earl S.: Studies on the Relation of the Lower Six-Year Molar to the Mandible. *Angle Ortho.* 10:24-32 Jan. 1940. Abstracted, *Angle Ortho.* 18:9 Jan.-April 1948.
4. Baldridge, John Philip: A Study of the Relation of the Maxillary First Permanent Molars to the Face in Class I and Class II Malocclusions. *Angle Ortho.* 11:100-109 April 1941. Abstracted, *Angle Ortho.* 18:10 Jan.-April 1948.
5. Brodie, Allan G.: Eighteen Years of Research at Illinois. *Angle Ortho.* 18:3-11 Jan.-April 1948.
6. Stoner, Morris M., Lindquist, John T., Vorhies, Jack M., Hanes, Rolenzo A., Hapak, Frank M., Haynes, Edgar T.: A Cephalometric Evaluation of Fifty-Seven Consecutive Cases Treated by Dr. Charles H. Tweed. *Angle Ortho.* 26:68-98 April 1956.
7. Wylie, Wendell L.: The Mandibular Incisor — Its Role in Facial Esthetics. *Angle Ortho.* 25:32-41 Jan. 1955.
8. Tweed, Charles H.: The Frankort-Mandibular Incisor Angle in Orthodontic Diagnosis, Treatment Planning and Prognosis, *Angle Ortho* 24:121-169, July 1954.

The Role Of The Soft Palate In Cleft Palate Speech

A. J. WILDMAN, D.D.S., M.S.D.

Eugene, Oregon

INTRODUCTION

A controversy exists at the present time concerning the etiology of defective cleft palate speech. Defective cleft palate speech is characterized by a harsh nasal quality which is often termed nasality. There is a tendency for defective cleft palate speakers to exhibit abnormal nasal emission of air when they attempt to build up and maintain air pressure for the production of speech sounds. There are two contradicting theories which attempt to explain the factors responsible for nasality and nasal emission in cleft palate speech.

Most observers seem to feel that nasal emission and nasality are the symptoms of an inadequate soft palate incapable of properly shutting off the nasopharynx from the oropharynx. The general tenets of this concept were first proposed by Passavant[1] in 1865.

Passavant felt that normal speech was impossible for an individual whose soft palate could not completely close off the nasal pharynx from the oral pharynx. He believed this closure was accomplished by the upward and backward movement of the soft palate against the bulging area on the posterior wall of the pharynx which is now called Passavant's pad, and by the medial movement of the lateral pharyngeal walls (Fig. 1 and 2).

Since Passavant's time, until very recently, most workers concerned with cleft palate rehabilitation have accepted the importance of an adequate nasopharyngeal closure to normal speech. Recently the validity of this entire concept has been challenged.

In 1951 McDonald and Baker[2] presented a new concept to explain defective cleft palate speech. They stated the ratio between the opening of the nasal and the oral port was the most important consideration determining the degree of nasality and nasal emission during speech in cleft palate individuals. They felt the competence of the velopharyngeal valve to be of secondary importance. They emphasized the importance of the size of the oral port which, they believed, was habitually reduced during speech in cleft palate individuals by the use of abnormally high lingual and mandibular positions.

Theoretically it should be very simple to test the validity of these two concepts. Measurements for nasality or nasal emission in a sample of cleft palate individuals could be plotted against measurements of soft palate function during nasopharyngeal closure. If Passavant was correct, the coordinates should approach a straight line indicating a high correlation between nasality or nasal emission and nasopharyngeal closure. If McDonald and Baker were correct, the coordinates should approach a random arrangement. Practically, this procedure is hampered by the difficulty of quantitatively measuring the variables involved.

Since nasality is an acoustic phenomenon, it must be measured by some device capable of quantitatively measuring the quality of speech sounds. In

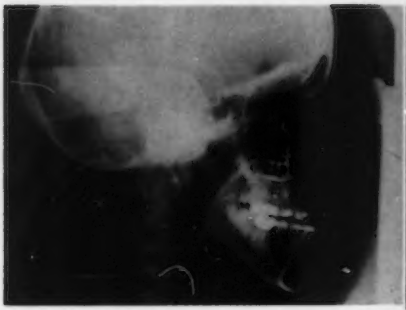

Fig. 1 Above: Lateral cephalometric radiograph of cleft palate individual taken during inhalation of air.

Below: Tracing of radiograph above. Note that the velopharyngeal valving mechanism is open.

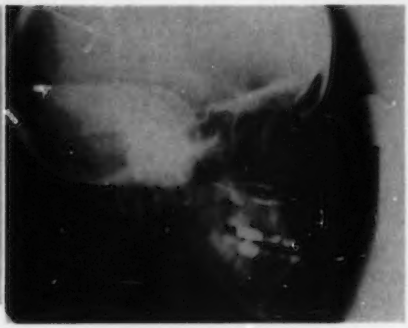

Fig. 2 Above: Lateral cephalometric radiograph of cleft palate individual taken during phonation of the vowel *u*.

Below: Tracing of radiograph shown above. Note that the velopharyngeal valving mechanism is in closed position.

1948 Kanter[3] stated that there were many devices which show nasality which were valuable as teaching aids. He warned that none of these was accurate enough to be of diagnostic value. At that time Kanter felt the human ear was still the most reliable instrument for measuring nasality. Quantitative measurement of nasality would probably be very difficult with equipment now available.

Nasal emission is a matter of air escaping from an orifice which theoretically can be measured in terms of air pressure. A number of devices have been used to measure nasal emission but none of them has been very satisfactory. One of the most encouraging attempts was made by Nusbaum, Foley and Wills.[4] They used a system of manometers to measure the pressure necessary to break down the velopharyngeal valve during the production of certain vowels. Their method was not suitable for measurement of nasal emission during the production of consonants. In 1955 Doubek[5] reported using new manometric methods to measure nasal emission. Since there are many simple air pressure measuring devices, it should not be too difficult to devise a satisfactory method of measuring nasal emission.

The positions of the soft palate can be adequately measured by cephalometric radiological techniques. A number of investigators have used cephalometric radiographs to study the position of the soft palate during the production of vowel sounds.

Although it might be very difficult to measure nasality, it seems that it should be possible to measure nasal emission and palatal movement during the production of speech sounds. A study designed to determine the correlation between nasal emission and palatal movement should shed much light on the etiology of defective cleft palate speech.

Purpose

The purpose of this study was to determine the relation between nasal emission and the movements of the soft palate during speech in a group of cleft palate individuals. Since an adequate instrument for measuring nasal emission was not available, it was necessary to adapt existing air pressure measuring devices for that purpose.

Method of Measuring Nasal Emission

Nasal emission was measured during the production of plosive and fricative consonants. Clinical experience indicates the greatest tendency toward nasal emission occurs when a cleft palate individual attempts to build up or maintain air pressure for the production of fricative or plosive consonants. The plosive sounds chosen for measurement were *pa, ta* and *ka*. The fricative sounds were *sa, sha* and *tcha*. The instrument developed to measure nasal emission in this study was called a nasometer, for lack of a better name, and was essentially a Marey tambour fitted with nasal olives and positioned to write on the smoked drum of a kymograph (Fig.3). Since the tambour readings were curvo-linear and were subject to changes in degree according to the condition of the rubber diaphragm, a U tube water manometer was used to calibrate the tambour before and after each test. The nasometer was operated in two phases. During the calibrating phase the end of the tambour connected with nasal olives was shut off and a column of air of given value was introduced into the tambour. This activated the tambour needle which produced a mark on the drum

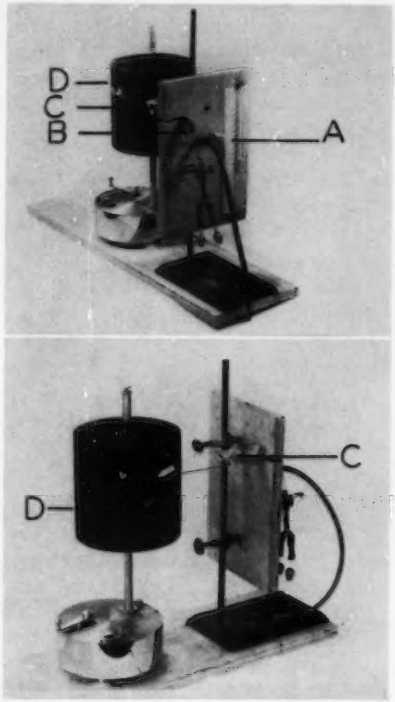

Fig. 3. The nasometer used to measure nasal emission of air.

A. The manometer in the calibrating system.

B. The hollow shunt from the calibrating system which delivers blasts of air of known force to calibrate the tambour.

C. Tambour.

D. Kymograph.

which represented a given air pressure. Four marks were made on the drum which represented one, two, three and four units of air pressure.

During the testing phase, the clamp between the nasal olives and the tambour was opened. The nasal olives were carefully fitted into the subject's nose and the subject was asked to repeat the consonant test sounds. Nasal emission of air accompanying the production of the speech sounds activated the tambour which produced test marks on the kymograph.

A microphone positioned three inches from the subject's mouth and connected to the volume control of the recording machine was used to control the sound level. Test sounds not produced on the standard level were discarded.

Nasal emission for each test sound was expressed in terms of the known calibrating units of air pressure in the following formula:

> Nasal emission in units equals the number of units represented by the largest calibrating mark equal to or immediately smaller than the test mark.

If the test mark in question was not exactly equal to a calibrating mark, then the fraction of the test mark was converted to units by the following formula:

> The length of the test mark minus the length of the calibration mark immediately smaller than the test mark divided by the length of the calibration mark immediately larger than the test mark minus the length of the calibration mark immediately smaller than the test mark.

The accuracy of the nasometer was tested by determining the ability of the instrument to produce the same readings in units of nasal emission for the same control test sounds produced by the same patient in two different tests. The machine was calibrated before each of the tests in the manner previously described. In each of the tests records were made by the previously described methods while the subject produced the sound *ta*. The nasometer was readjusted, the diaphragm of the tambour was changed and the machine was again calibrated in the interim between the two tests.

The mean for group one was 23.5 units and the mean for group two was 22.0 units. When the T ratio was calculated, it was found $T = 1.378$, which indicated there was no significant difference in the two samples at the 5% level.

Method of Measuring Functional Movements of the Soft Palate

Cephalometric radiology lends itself ideally to the study of functional movements of the soft palate and associated structures. Oriented cephalometric radiographs are taken with the aid of a head-positioning device which holds the patient in either a labial or anterior position at a fixed distance from the x-ray tube. Since the position of the x-ray tube and subject is fixed, it is possible to produce comparable radiographs at desired intervals of time. A number of workers have used lateral cephalometric radiographs successfully to study the movements of the soft palate. A radiopaque disclosing solution is often used to define the soft palate and associated soft tissue structures of the pharynx.

Ideally, a lateral cephalometric radiograph should have been taken during each of the consonant sounds for which a score for nasal emission was obtained. However, the movements of the palate are rapid enough during the production of consonants to make the production of good radiographs difficult. On the other hand, during the production of vowels the palate moves

to its position of function and stays in that position over some period of time. Most investigators agree that the soft palate is relatively open during the production of such vowels as *a* but closed during the production of *i* and *u*. Williams[6] and Carpenter[7] used cephalometric radiographs of the palate at rest and during phonation of the vowel *u* to measure velar function. That was the technique decided upon for this study.

Iodo-chlorol, a radiopaque disclosing solution, was used to facilitate the visualization of the soft tissues of the velopharyngeal valving mechanism in the lateral headplates.

Superimposed tracings were made for each patient from lateral cephalometric headplates taken during the phonation of the vowel *u* and during the inhalation of air into the lungs. The tracing of the headplate taken during inhalation, which represented the resting position of the palate, was done first. That tracing was then superimposed on the headplate of the same subject taken during the phonation of the vowel *u*, which represented the palate in the closing state, and the soft palate was traced on as a broken line. The composite tracing represented the palate functioning in velopharyngeal closure and in the resting position (Fig. 4).

A reference line was drawn on the composite tracing through the area where the velum and the palate most nearly approximated during valving. The following reference points were established:

W—The point at which the reference line bisected the posterior wall of the pharynx in the resting position.

X—The point at which the reference line bisected the posterior wall of the pharynx during function.

Fig. 4. Composite lateral cephalometric tracing showing velopharyngeal valving. YZ represents soft palate movement during velopharyngeal valving. WX represents postpharyngeal movement.

Y—The point at which the reference line bisected the line of the functioning velum.

Z—The point at which the reference line bisected the line of the resting velum.

The length of the lines WZ, WX and YZ were measured in mm. with the aid of fine line dividers and a mm. rule. The following ratios were computed for each case: YZ:WZ and WX:WZ. The ratio YZ:WZ was said to indicate the contribution of the posterior movement of the velum to closure of the velopharyngeal valve aperture. The ratio WX:WZ was said to indicate the contribution of the anterior wall of the pharynx to closure of the velopharyngeal valve aperture. The line WZ was said to represent the antero-posterior aperture of the velopharyngeal valve.

Data

The data were arranged to show the relation between nasal emission and velopharyngeal closure in the group studies.

Linear correlation coefficients were determined between the scores for nasal emission and the scores obtained by measuring the velopharyngeal closure

by three different methods.

In Figure 5, above, the mean values for nasal emission recorded during the production of the speech sounds *pa, ta, ka, sa, sha* and *tcha* were compared to the degree of movement of the velum toward the posterior wall of the pharynx during phonation of the vowel *u*. The value for r was —0.753. The linear correlation of the two groups was significant at the 1% level. The strong negative relation indicated that the subjects whose velums accomplished the greatest percentage closure of the velopharyngeal aperture during the phonation of the vowel *u* showed the least degree of nasal emission during the production of the sounds *pa, ta, ka, sa, sha* and *tcha*.

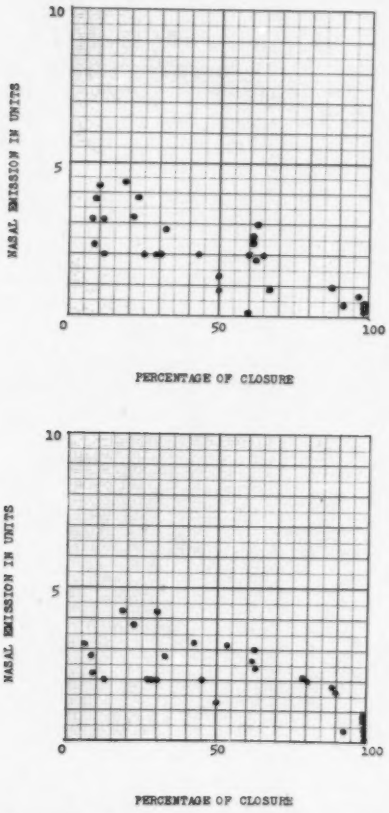

Fig. 5. Above: Scattergraph showing strong correlation between nasal emission and the amount of palatal movement during velopharyngeal closure.

Below: Scattergraph showing the strong correlation between nasal emission and the amount of palatal and post-pharyngeal movement during velopharyngeal closure.

Figure 5, below, shows the relation between the mean nasal emission recorded during the production of the sounds *pa, ta, ka, sa, sha* and *tcha* and the combined contribution to velopharyngeal valving of the movement of the velum toward the posterior wall of the pharynx, and the movement of the posterior wall of the pharynx toward the velum, during the phonation of the vowel *u*. The value of —0.775 for the linear correlation coefficient indicated a negative linear correlation significant at the 1% probability level. The strong negative relation indicated the subjects whose velopharyngeal apertures were closed to the greatest percent by the movement of the posterior wall of the pharynx and the velum during the phonation of the vowel *u* showed the least nasal emission during the production of the sounds *pa, ta, ka, sa, sha* and *tcha*.

Figure 6 shows the relation between the mean nasal emission recorded during the production of the sounds *pa, ta, ka, sa, sha* and *tcha* and the distance between the velum and the posterior pharyngeal wall measured at the narrowest part of the velopharyngeal aperture during the phonation of the vowel *u*. The value for the linear correlation coefficient is +0.743 which is significant at the 1% probability level. The strong positive correlation indicates the subjects with the smallest measurements of the velopharyngeal valve aperture showed the least degree

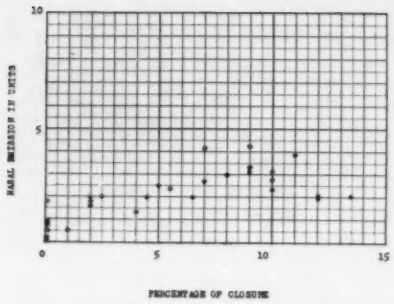

Fig. 6. Scattergraph showing strong correlation between the degree of nasal emission and the size of the aperture between the soft palate and post-pharyngeal wall during velopharyngeal closure.

of nasal emission during the production of the sounds *pa, ta, ka, sa, sha* and *tcha.*

Discussion

The data show an extremely high degree of correlation between nasal emission and velopharyngeal closure. Those individuals with the most efficient velopharyngeal valving mechanisms showed the least tendency toward nasal emission.

These findings support Passavant's contention that a mobile soft palate capable of shutting off the nasopharynx from the oropharynx is essential to normal speech for cleft palate individuals. The data do not support McDonald's and Baker's belief that the amount of nasal emission in cleft palate speech is related only secondarily to the degree of patency of the nasopharyngeal port.

The very high degree of correlation found in this study between nasal emission and velopharyngeal valving efficiency indicates the method of testing these variables was accurate. If inaccurate measurements approaching a random sample were obtained for either nasal emission or velopharyngeal function, then it would have been impossible to show such a high degree of linear correlation between these two variables. The cephalometric techniques for measuring palatal function and the nasometer appear to be accurate.

One of the diagnostic criteria of velopharyngeal valving efficiency used today is the position of the soft palate shown in lateral cephalometric radiographs during the phonation of the vowel *u*. The successful use of this technique to measure palatal function in this study should encourage clinicians working with cleft palate cases to use cephalometric radiology in diagnosis with increased confidence. The use of cephalometric radiology to follow cleft palate cases by surgeons, speech therapists and other members of cleft palate teams seems to be justified.

Clinical methods of measuring nasal emission have in the past been very unsatisfactory. The nasometer used in this study proved to be a sensitive and reliable research tool and should be valuable as a clinical method of measuring nasal emission. One obvious clinical application of the nasometer would be its use to follow the progress of cleft palate patients. Measurements could be taken before and after staphylorrhaphy to determine any immediate improvement in the patient's ability to control nasal emission. Measurements could then be taken with the nasometer at regular intervals during the progress of speech therapy to determine how successfully the patient was learning to control nasal emission.

Diagnosis and treatment of defective cleft palate speech is complicated at best. Perhaps in time we will have an adequate armamentarium of accurate instruments and a solid base of theoretical knowledge to help us meet the challenge by cleft palate patients who need our help.

Summary and Conclusions

This study was designed to determine the relation between nasal emission of air during speech and the efficiency of the velopharyngeal valving mechanism in cleft palate individuals. The group used in this study consisted of thirty male and female cleft palate patients, of an age range from ten to twenty years, from Northwestern University's Cleft Palate Institute. Nasal emission was measured during the phonation of the consonants *pa, ta, ka, sa, sha* and *tcha* by an instrument called a nasometer which was developed for this study. The movements of the soft palate and posterior wall of the pharynx were measured from tracings of lateral cephalometric radiographs taken during inhalation and during phonation of the vowel *u*. Evaluation of the results suggested the following interpretations and conclusions.

1. The linear correlation between the scores for nasal emission and the scores for velopharyngeal valving were significant at a level well below the 1% probability level. This evidence indicates that the soft palate plays an important role in the control of nasal emission by the cleft palate individual.
2. The nasometer developed for this study proved to be a reliable research instrument. Clinicians may find the nasometer a valuable clinical tool for evaluating and following cleft palate cases.
3. The use of lateral cephalometric radiographs as a diagnostic criteria to determine the efficiency of the velopharyngeal valving mechanisms seems to be justified.

1200 High Street

Bibliography

1. Passavant, Gustav, "Ueber die Beseitigung der naeselden Sprache bei angeborenen Spalten des harten und weichen Gaumens (Gaumensegel, Schlundaht und Ruecklagerung des Gaumensegel)", *Arch. klin. Chir.*, 6:333, 1865.
2. McDonald, E.L.T. and Koepp-Baker, H., "Cleft Palate Speech", *J. Speech and Hearing Disorders*, 16: 9-20, 1951.
3. Kanter, C. E., "Diagnosis and Prognosis in Cleft Palate Speech", *J. Speech and Hearing Disorders*, 13:211-233, 1948.
4. Nusbaum, E. A., Foley L., and Wills, C., "Experimental Studies of Velopharyngeal Occlusion During the Production of English Vowels", *Speech Monogr.*, 2:71-80, 1935.
5. Doubek, F., "Die Prufung der Sprechfunktion bei Gaumenspaltenoperationen", *Fortschr. Kief. Ges. Chir.*, 1:104-111, 1955.
6. Williams, R. L., "A Serial Radiographic Study of Velopharyngeal Closure and Tongue Position in Certain Vowel Sounds", M.S.D. Thesis, Northwestern University Dental School, 1951.
7. Carpenter, C. R., "A Serial Radiographic Study of Velopharyngeal Closure and Tongue Position in Certain Vowel Sounds", M.S.D. Thesis, Northwestern University Dental School, 1954.

Hereditary Factors In Tooth Dimensions, A Study Of The Anterior Teeth Of Twins*

SIDNEY L. HOROWITZ, D.D.S.
RICHARD H. OSBORNE, Ph.D
FRANCES V. DEGEORGE, M.A.
New York City

INTRODUCTION

It has long been assumed that tooth breadth is a characteristic controlled by hereditary forces. Kingsley[1] expressed one of the prevailing orthodontic concepts of the nineteeth century when he wrote, "The cause of irregularities of teeth is . . . sometimes due to the inheritance of large teeth out of all proportion to the size of the inherited jaw." This view is still widely held today. It is generally accepted that, while harmonious relationship between tooth dimensions and arch length results in the development of a satisfactory alignment and optimum occlusion of the teeth, disproportion between the two elements predisposes toward crowding or spacing in the arch[2].

Greater emphasis has been placed on assessing the ratio, or proportional relationship, between the size of teeth and the length of the bony arch [2 9], and on the relationship between the upper and lower teeth[10 12], than upon the hereditary aspects of either tooth or bony arch dimensions. As these analyses have in common the evaluation of differences in relative proportions, either of teeth to the bony arch or of teeth to each other, it becomes of fundamental interest to establish biometrically the range of variation of each factor in the ratio.

The quantitive variation which occurs in a complex morphological characteristic such as mesio-distal tooth dimension is influenced by both hereditary and environmental forces. The effects of a non-genetic factor (diet) on the size of molar teeth in rats has recently been demonstrated by Paynter and Grainger[14] and indicates that this is one environmental aspect of tooth development which requires further investigation.

The present communication is concerned with the analysis of the quantitative hereditary variation which occurs in one element of the tooth size/bony arch ratio, the mesio-distal tooth dimensions of the six maxillary and six mandibular anterior teeth.

THE STUDY METHOD

The twin-study method has been used in the investigation of hereditary characteristics in man since Galton's time[15]. The method has been employed occasionally in dental and orthodontic research, perhaps most frequently for the investigation of inherited variation in tooth size and occlusion[16 20].

By definition, a study of tooth dimensions is a study of quantitative variation. The problem is quite different than one of whether a given tooth is or is not inherited as might be the case, for example, in a qualitative study of

*Division of Orthodontics, School of Dental and Oral Surgery of the Faculty of Medicine, and Institute for the Study of Human Variation, Columbia University, New York.

congenital tooth absence. Rather, it is a matter of whether the individual variations in tooth dimensions have a measurable hereditary component. In order to evaluate this type of a genetic problem ideally, an investigative procedure is required in which genetic variation is a known factor, and in which other contributory variables can either be controlled or measured.

The twin study method more nearly satisfies these exacting requirements than any other method presently available. In comparing the two members of monozygotic twin pairs whose coefficient of genetic relationship is (by definition) equal to 1.0, indicating identical genetic endowment in both members of the pair, any observed difference in tooth dimension must be due to factors other than heredity.

When both members of a monozygotic twin pair are studied simultaneously, differences in developmental stage are eliminated; as monozygotic twins must be of the same sex, sex differences are automatically excluded. In pairs where both members have been raised in the same home, major nutritional and other environmental factors are similarly reduced to a practical minimum for human studies. The remaining causes of variation become developmental or health differences, which can be determined from observing other physical attributes and by obtaining comparable health (including dental) and developmental histories, and measurement on experimental error.

In the case of dizygotic twins, the average coefficient of genetic relationship is equal to 0.5, which is the same as that for ordinary single-born siblings. Consequently, observed differences between the two members of a dizygotic twin pair result from the same influences which affect monozygotic twins plus a difference in one-half of their total heredity. When a study of dizygotic twins is carried out to parallel the one outlined above for monozygotic twins, a comparison of the average of the differences for monozygotic and dizygotic twins will provide a test of the observed effect of a difference in one-half the total hereditary constitution.

The Study Sample

The subjects utilized for the present investigation are from a larger study (to be reported in detail elsewhere) which has been in progress for several years at the Columbia-Presbyterian Medical Center[21].

The total study group consists of Caucasian twins drawn from a variety of sources in New York City, unselected as to sex and zygosity, with an age range from 18 - 55 years, and obtained for the purpose of establishing a sample of adult twin subjects in good general health.

Tooth dimensions were obtained for 54 pairs of like-sexed twins with an average age of 27 years. This group included 33 pairs of monozygotic twins (21 female and 12 male), and 21 pairs of dizygotic twins (16 female and 5 male).

Methods

The central problem in any twin study is the accurate diagnosis of zygosity. The method employed in the present study was based upon proving *dizygosity*, first by a proven difference in a blood group factor, and in addition by other reliable characteristics such as eye color, ear form and dermal patterns for all like-sex pairs which agreed in all their blood factors. The advantages of this method are: (1) it provides a known direction to any possible error in diagnosis, and (2) it tends to underestimate rather than overestimate the inheritance factor for any

characteristic under investigation. It may be assumed, therefore, that the conservative nature of the method provides ample insurance that significant results are not attributable to chance errors in the diagnosis of zygosity. A detailed discussion of this method may be found elsewhere[23]. *Measurement Methods.* Quick setting stone casts were prepared from alginate impressions of the maxillary and mandibular teeth of each subject. Measurements of the widest mesio-distal diameter of each of the six maxillary and six mandibular anterior teeth of the permanent dentition were obtained from the models by means of a finely pointed sliding caliper fitted with a vernier scale which permitted readings to the nearest 0.1 mm. The legs of the caliper were placed at the contact points and the instrument held parallel to the incisal surfaces of the teeth as the individual tooth was measured. Teeth which had been restored with porcelain or acrylic jackets, and those which had either carious lesions or restorations which affected a contact point were eliminated from the study.

Duplicate measurements were made of all of the twelve teeth studied utilizing the models of thirty subjects selected at random. These double determinations were used to assess the measurement error.

Results

Monozygotic twins. The average differences observed between the members of monozygotic twin pairs are given in Table 1. In the maxillary arch it is the left central which shows the smallest mean difference, 0.17 mm., while the left lateral has the greatest, 0.24 mm. For the mandibular teeth these differences range from 0.10 mm. for the left central to 0.21 mm. for the left canine.

Dizygotic twins. Table 1 also presents the average differences observed in the

TABLE I

Comparison of Mean Intra-pair differences in Mesio-Distal Tooth Dimensions between Monozygotic and Dizygotic twins.

Maxillary Teeth

	n (pairs)	Mean diff. (mm)	S.D.	P†
Right				
I_1				
Monozygotic	.30	.20	.18	<.001*
Dizygotic	...18	.37	.34	
I_2				
Monozygotic	.29	.22	.17	>.001*
Dizygotic	...18	.56	.59	
C				
Monozygotic	.29	.23	.15	<.025
Dizygotic	...18	.33	.26	
Left				
I_1				
Monozygotic	.31	.17	.14	>.001*
Dizygotic	...16	.37	.28	
I_2				
Monozygotic	.31	.24	.20	>.005*
Dizygotic	...16	.42	.33	
C				
Monozygotic	.26	.19	.15	<.25
Dizygotic	...14	.14	.18	

Mandibular Teeth

	n (pairs)	Mean diff. (mm)	S.D.	P†
Right				
I_1				
Monozygotic	.26	.13	.12	>.001*
Dizygotic	...15	.29	.29	
I_2				
Monozygotic	.29	.16	.11	>.001*
Dizygotic	...17	.35	.33	
C				
Monozygotic	.29	.20	.14	<.001*
Dizygotic	...17	.38	.24	
Left				
I_1				
Monozygotic	.27	.10	.08	>.001*
Dizygotic	...18	.35	.26	
I_2				
Monozygotic	.28	.14	.16	>.005*
Dizygotic	...15	.30	.22	
C				
Monozygotic	.26	.21	.21	<.05
Dizygotic	...21	.31	.25	

†A variance analysis was employed and P values were obtained by means of Fisher's F test (variance ratio).

*Statistically highly significant.

tooth breadths of the dizygotic twins. The maxillary teeth show a range of differences from 0.14 for the left canine to 0.56 for the right lateral. In the mandible the range is from 0.30 mm. for the left lateral to 0.38 for the right canine.

Comparison of the standard deviations provides a familiar estimate of the variability of the mean differences obtained between the members of the twin pairs. For the monozygotic twins the S.D. of the six maxillary teeth show a small range, from 0.14 to 0.20; while the mandibular S.D. vary between 0.08 and 0.21.

In the dizygotic twins the S.D. of the maxillary teeth fall into a pattern. Those for the central and lateral teeth show evidence of appreciably greater variability than either of the canines. In the lower arch this also holds true on the right side, while on the left the canine forms an exception.

Difference Between Categories

Comparison of the mean intra-pair differences observed between members of the twin pairs in the separate categories provides a measure of the degree to which monozygotic twins are more alike than dizygotic twins. The larger the difference between these two values, the greater is the hereditary factor in the variability of the particular tooth. As has been noted, none of the teeth measured show large mean variations between the members of monozygotic twin pairs. The mean differences are much greater between members of dizygotic twin pairs.

By employing a variance analysis based upon the mean intra-pair differences it becomes possible to obtain an efficient and statistically exact test of the significance of these differences. Fisher's variance ratio, or F test, has been used for tests of statistical significance in this study. (For details of the statistical methods employed see Osborne[22], et. al.)

When such a test is applied, the most prominent feature of these data is the strong genetic component of variability of the four maxillary and four mandibular incisor teeth. The differences between members of monozygotic and dizygotic twin pairs are statistically highly significant for all of these teeth. The probability values range from $P = >.005$ to $P = <.001$.

The canine teeth, on the other hand, give evidence of much less of an hereditary component of variability. The mean intra-pair values do not show statistically significant intra-pair differences between the twin categories with the single exception of the mandibular right canine. This low measurable hereditary variability, especially in the upper canines, is not the result of an increase in non-genetic (environmental) influence, but reflects an actual lack of genetic variability observed in these teeth between the members of dizygotic twin pairs.

Environmental Variation

Non-genetic variation for each of the twelve teeth studied may be further analyzed by comparing the mean differences observed between the members of monozygotic twin pairs. Environmental variation is about twice as great in the maxillary central and lateral incisors as in the corresponding mandibular teeth. The canine teeth demonstrate non-genetic variability comparable to that of the incisors.

Sex and asymmetry components affect non-genetic variability and may also be measured by comparing the monozygotic twins (Table 2). It is apparent from the data that these factors accumulate particularly in the maxillary left central, mandibular left canine and mandibular left lateral, resulting

TABLE II

Male - Female Intra-pair Differences in Mesio-Distal Tooth Dimensions (Monozygotic Twins)

	RIGHT				LEFT			
	n (pairs)	mean difference (mm)	S.D.	P	n (pairs)	mean difference (mm)	S.D.	P†
Maxillary Teeth								
I_1								
Male	12	.23	.17	<.25	12	.26	.15	<.005*
Female	18	.18	.19		19	.12	.10	
I_2								
Male	10	.27	.17	>.25	12	.23	.25	<.25
Female	19	.20	.16		19	.24	.16	
C								
Male	10	.23	.19	<.25	11	.16	.16	<.25
Female	19	.24	.13		15	.21	.14	
Mandibular Teeth								
I_1								
Male	9	.19	.12	<.05	11	.11	.08	.25
Female	17	.09	.11		16	.09	.07	
I_2								
Male	11	.15	.10	<.25	10	.17	.22	.025
Female	18	.12	.11		18	.12	.12	
C								
Male	10	.18	.16	<.25	8	.34	.25	.005*
Female	19	.21	.14		17	.14	.14	

in statistically significant sex differences in those teeth. Our data indicate that this is unilateral in nature and tooth specific.

Discussion

Earlier odontometric studies of twins give an approximate indication of the variability of the anterior teeth, but tests of statistical significance cannot be made from the data as published. Nevertheless, both Reif[16] and Korkhaus[19] found the largest intra-pair differences in size (by percentage) occurred in the maxillary right lateral teeth. This genetic variability is confirmed in our data with a high degree of statistical significance.

Lundström[20], in his twin study, also obtained the largest coefficient of variation in the maxillary lateral incisors and additional evidence of the great variation in mesio-distal width of the laterals is provided by odontometric population studies[3 23 25].

By comparison with the incisor group, the canine teeth demonstrate very little measurable genetic variability. This finding confirms previous reports. Lundström's twin data indicate a lower coefficient of variation for the canines than for the incisor teeth and odontometric population studies agree on the low degree of canine variability.

The comparatively low genetic component of variability demonstrated in the canine tooth width is of interest in numerous ways. Functionally, this tooth occupies a strategic location in the dental arch as the link which connects the premolar-molar series with the incisors. In prosthetic dentistry it is an extremely important dental unit owing to its large area of periodontal membrane attachment. In addition, the maxillary canine has an esthetic function

in preserving facial contours, acting as a support for the corner of the lip. To the orthodontists, the canine is the "keystone in its lateral half of the dental arch[26]," a role which demands relative constancy in size and form. Dahlberg[27 28], in his adaptation of Butler's[29] field concept of the human dentition, considers the canine "morphologically stable as concerns expression and retention of ancestral patterns." The limited genetic variability of the canine observed in this study is compatible with Dahlberg's hypothesis of a comparatively slow rate of evolutionary change, or "stability" in this tooth.

With regard to bilateral asymmetry noted in the data, it appears that the environmental variability of teeth on the left side, particularly the maxillary central, mandibular canine and mandibular lateral, is sex-influenced. It is the significantly greater male variability which produces this right-left discrepancy in the mandibular canine ($P = .025$ when the right and left sides are compared). That the sex influence operates more strongly on the left side has also been found in dermatoglyphic patterns[30], which suggests that there may be some general sex-influenced asymmetry mechanism operating at a developmental level. Significant sex differences in canine tooth dimensions have been reported in recent population studies[24 25].

Analysis of the data reported offers a partial explanation for the inconsistent results in treatment planning obtained with various tooth size/bony arch ratios[9 31]. As has been pointed out[6], it may not be possible to attain high precision in predictive methods based on tooth size. When mesio-distal tooth dimensions are used to establish orthodontic diagnosis ratios, the findings of this study suggest that the anterior teeth be considered as two separate groups, the "variable" incisors and the relatively "stable" canines.

Summary

1. Hereditary variation in mesio-distal tooth dimensions is analyzed by means of the twin-study method, utilizing a group of fifty-four pairs of adult, like-sexed Caucasian twins.

2. Genetically conditioned variations of a highly significant nature occur in eight of the twelve anterior teeth studied. The canine teeth demonstrate a relatively low hereditary component of variability.

3. Sex and asymmetry factors appear to play a part in the variation observed in the maxillary left central, and mandibular left canine and lateral teeth.

630 West 168th St.

References

1. Kingsley, N. W. A Treatise on Oral Deformities as a Branch of Mechanical Surgery, New York, Appleton, 1888.
2. Moorrees, C. F. A. and R. B. Reed Biometrics of Crowding and Spacing Teeth in the Mandible. *Amer. J. of Physical Anthro.* n.s. 12: 77-88, 1954.
3. Seipel, C. M. Variation of Tooth Position. *Svensk Tandläkare - Tidskrift* 39: supplement, 1946.
4. Howes, A. E. Case Analysis and Treatment Planning Based Upon The Relationship of the Tooth Material to Its Supporting Bone. *Amer. J. of Ortho. and Oral Surg.* 33: 499-533, 1947.
5. Nance, H. M. The Limitations of Orthodontic Therapy. *Amer. J. of Ortho. and Oral Surg.* 33: 177-233, 253-301, 1947.
6. Ballard, M. L. and W. L. Wylie Estimating the Size of Unerupted Teeth. *Amer. J. of Ortho.* 33: 754, 1947.
7. Carey, C. W. "Linear Arch Dimension and Tooth Size." *Amer. J. of Ortho.* 35: 762, 1949.
8. Lundstrom, A. Äro trangställninger vanligare hos fall med stora än med sma tänder? *Svensk Tandläkare - Tidskrift,* 35: 399, 1942. (as quoted by Moorrees and Reed, 1954)
9. Rees, D. J. A Method For Assessing the Proportional Relation of Apical Bases and Contact Diameters of the Teeth. *Amer. J. of Ortho.* 39: 695-707, 1953.
10. Tonn, P. über die mesio-distalen Zahnbrieten - Relationen der Zähne - des Oberkiefers zu den entsprechenden des

Unter-Kiefers bei normaler und abnormaler Okklusion. - Diss. Berlin, 1937, (As qouted by Seipel, 1946).

11. Steadman, S. R. The Relation of Upper Anterior Teeth to Lower Anterior Teeth as Presented on Plaster Models of a Group of Acceptable Occlusions. *Angle Ortho.* 22:91, 1952.
12. Lundström, A. Intermaxillary Tooth Width Ratio and Tooth Alignment and Occlusion. *Acta Odontologica Scandinavica* 12: 265, 1954.
13. Neff, C. W. The Size Relationship Between the Maxillary and Mandibular Anterior Segments of the Dental Arch. *Angle Ortho.* 27: 138, 1957.
14. Paynter, K. J. and R. M. Grainger The Relation of Nutrition to the Morphology and Size of Rat Molar Teeth. *J. of the Canadian Dental Asso.* 22: 519-531, 1956.
15. Galton, Sir Francis The History of Twins as a Criterion for the Relative Powers of Nature and Nurture. *Pop. Sci. Monthly* 8: 345, 1875.
16. Reif, W. Die Vererbung der Kronenform menschlicher Zähne. Inaug. Diss. Bonn, 1928 (as quoted in Lundström, 1948).
17. Bratengeier, E. Über die Variabilität der Frontzähne bei ein-und zweieiigen Zwilligen. Med. Diss. Frankfort, 1934 (as quoted in Lundström, 1948).
18. Goldberg, Samuel Biometrics of Identical Twins from the Dental Viewpoint, *J. of Dental Research* 9: 363, 1929.
19. Korkhaus, G. Die Vererbung der Kronenform und grösse menschlicher Zähne. Zeitschrift für Anatomie und Entwicklungsgeschichte. 91: 594, 1930.
20. Lundström, A. Tooth Size and Occlusion in Twins. Karger, Basle, 1948.
21. Osborne, R. H., Hereditary and Environmental Factors in Body Build: A Study of 100 Pairs of Adult Twins. Ph.D. Thesis Columbia University. 1956.
22. Osborne, R. H., S. L. Horowitz and F. V. DeGeorge. A Twin Study of Variation in Tooth Dimension. In preparation.
23. Greiwe, Paul W. Tooth Size and Symmetry in the Human Dentition, M. S. Thesis, Department of Orthodontics, Graduate College of the State University of Iowa, 1949.
24. Thomsen, Selma, Dental Morphology and Occlusion in the People of Tristan da Cunha. Results of the Norwegian Scientific Expedition to Tristan da Cunha, 1937-38 No. 25. Publ. Det. Norske Videnskaps - Akademi, I Oslo. 1955.
25. Moorrees, C.F.A., Selma Thomsen, Elli Jensen and Peter Kai-Jen Yen. Mesiodistal Crown Diameters of the Deciduous and Permanent Teeth in Individuals. *J. Dent. Research* 36: 39, 1957.
26. Angle, E. H. Treatment of Malocclusion of the Teeth Phila. *S. S. White* 7th Ed., 1907.
27. Dahlberg, Albert A. The Changing Dentition of Man. *J. Amer. Dental Asso.* 32: 676, 1945.
28. Dahlberg, Albert A. The Dentition of the American Indian. Papers on the Physical Anthropology of the American Indian p. 138 *Viking Fund*, N. Y., 1949.
29. Butler, P. M. Studies of Mammalian Dentition. Differentiation of Post-Canine Dentition. *Proc. Zool. Soc. London* 109: 1-36, 1939.
30. Turpin, R. and M. P. Schutzenberger, On a Lateral Statistical Asymmetry Attribute of the Human Sexual Phenotype. *Compt. Rend. Acad. Sci.* (Paris) 228(5): 431, 1949.
31. Martinek, C. E. A Comparison of Various Surveys on the Adequacy of Basal Bone. *Amer. J. of Ortho.* 42: 244-254, 1956.

Early Treatment

HENRY A. STURMAN, D.D.S., M.S.

Hartford, Conn.

When Angle advanced his all-embracing biologic concept of normal occlusion, he provided all of dentistry with a firm foundation upon which to rear its edifice. He further strengthened that foundation for the orthodontist, in particular, when he formulated his definition of the Line of Occlusion. Here was no static and unchanging structure or system, but rather one that reflected all of the vital propensities of life — its arrival at the human stage of evolution and its ability to adapt itself in each individual to those modifying influences that result from its inheritance.

The teeth and their occlusion still have as their main function the comminution of food, but that portion of the face which supports them has come to assume a wide variety of other functions. The teeth are immutable once they are formed and, unlike almost all other tissues and organs, have no ability to repair or adjust themselves. Set in labile bone, they are passive victims of all of the forces that play upon them. Heredity, the timing of growth, the vigor of function, and even the instability of those most recent acquisitions, the muscles of expression, are the true determinants of the occlusion. Both normal and abnormal occlusion represent an equilibrium of all of these forces *at any given instant of time*. Our gravest errors as orthodontists, however, arise through our failure to remember this last point. What a denture represents at any instant of time gives only a clue as to what it has been or what it might be in the future. To defer treatment until almost the entire denture is developed, as is so generally done, is to ignore the basic fact that children grow and by our neglect we fail to take advantage of the help that such growth can give us.

In Figures 1 and 2 are shown the models of two cases in which early simple treatment resulted in excellent dentitions. The first was a Class I malocclusion with marked deficiency of the incisor region, both mandibular and maxillary, coupled with a deep overbite.

A bite plane was made for this patient at seven years of age and was worn intermittently for two years with the result shown. No other treatment was ever necessary.

The case illustrated in Figure 2 was a Class II malocclusion when first seen at eight and one half years. The face had a decided Class II, Div. I appearance. In this case a bite plane in conjunction with cervical traction was used for a period of two years, and no further treatment was necessary.

I anticipate the arguments that will immediately be brought against this more optimistic point of view; viz., that the size of man's jaws is diminishing at a faster rate than that of his teeth; that background radiation is raising hob with his mutation rate, and all the rest of the "scientific" evidence to which we have been subjected and which we have swallowed "hook, line, and sinker" in a desperate effort to excuse our ig-

Presented at the reunion meeting of the Graduate Department of Orthodontia, University of Illinois, March 5, 1956, Chicago, Illinois.

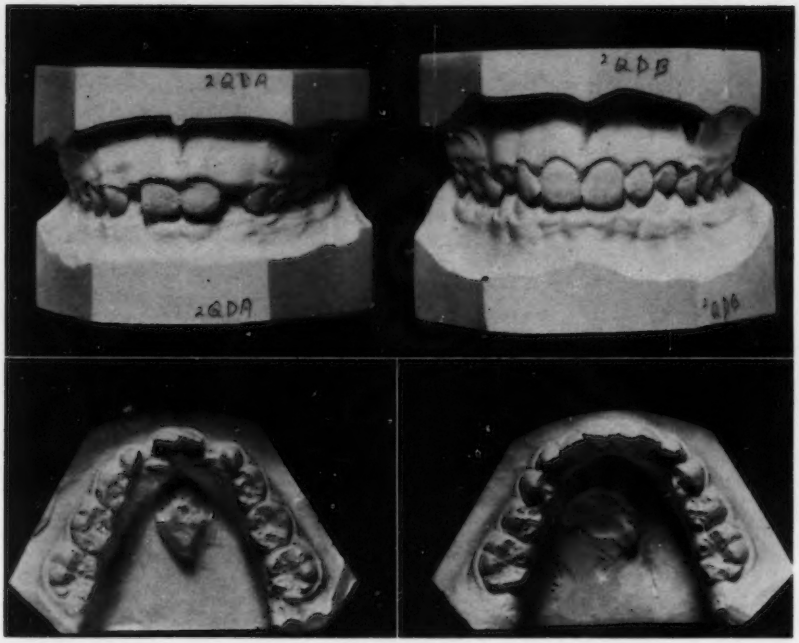

Fig. 1 Models of patient treated only with bite plane. Models on left show the occlusion at the start when patient was seven years old; those on the right show the case after intermittent wearing for two years.

norance, rationalize our procedures and avoid any unnecessary or serious cerebration of our own.

By the way of rebuttal, however, I should like to point out that no mention is ever made of the opposite side of these questions; viz., the frequency of congenital absence of teeth or of spaced dentures without such absences; that the significance of those recent researches that disregard that sacred cow "statistics" and reveal the individual as unique in the truest sense of the word has not yet dented our collective consciousness. Finally, I should like to call attention to the fact that dried skull evidence of adult material is a poor source of information upon which to base any sweeping conclusions as to the phylogenetic destiny of Man. Experience with living, growing children quickly reveals that the potential for adequate growth and normal occlusion is still present in the modern child, although, perhaps, more frequently thwarted or even inhibited by relatively minor factors. It seems to me that it is our duty as scientific men to delve more deeply into this aspect of our work instead of resigning ourselves to an attitude of compromise with what we mistakenly think of as inevitable limitations.

Now, I am not counseling a return to our former naive belief that every child is born with a potential for well-integrated and adequate development. The three faces illustrated in Figure 3 indicate the lack of good integration among the variables that go to make

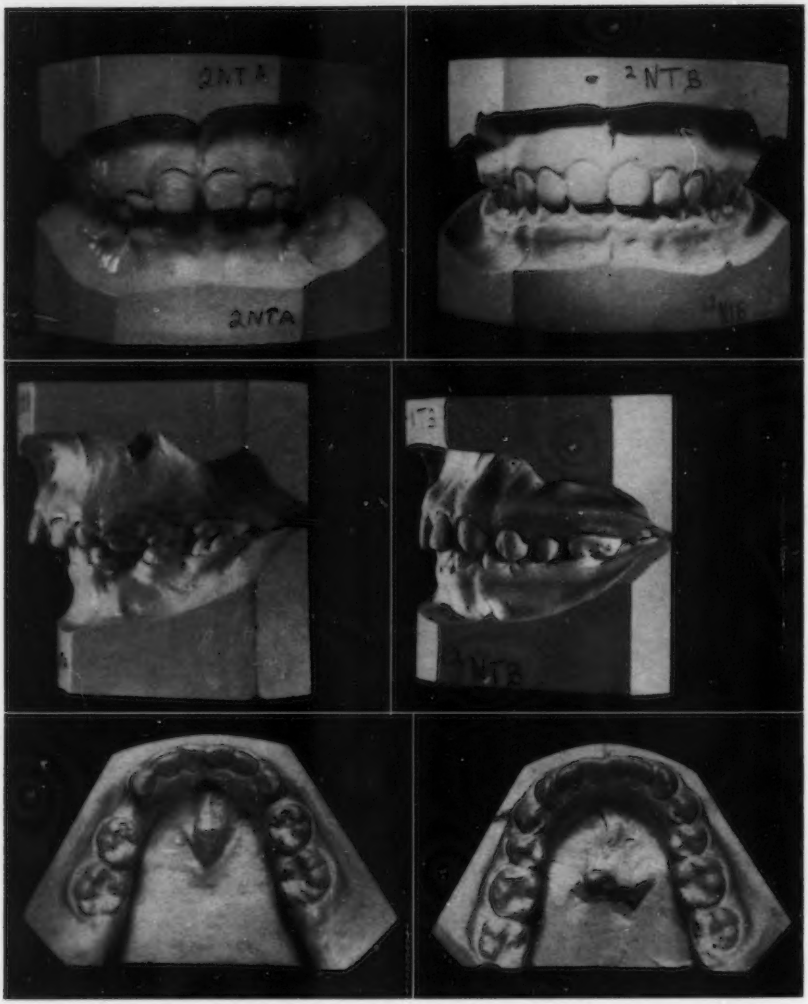

Fig. 2 Models of patient treated with only bite plane and cervical traction. Models on left show occlusion at the start at eight and one half years; those on the right show the case after two years' management with bite plane and cervical traction.

up the face. The fault, of course, lies in the genes which in these cases gave rise to parts that were too far out of the usual range to permit harmonious adjustment. When we consider that this factor of variation (due to lack of genetic harmony) exists throughout the long range from such monstrosities to the ideal, and that there are a large number of such variants interacting upon each other, we should not be surprised at a slipped contact occasionally.

The two cases illustrated in Figures 4 to 7 inclusive indicate different sets of conditions. In both, the prognosis would appear to be very poor in the first records. In the first (Figs. 4 and 5) there was a severe Class II, Div. I malocclusion complicated by lingually blocked lower lateral incisors at nine years of age. Obviously the jaws were growing slowly and the dentition was even further delayed.

Treatment consisted of applying extraoral anchorage to the upper first permanent molars until a Class I relation was established. In the meantime the lower deciduous canines were lost with some of the space they had occupied, and cervical traction was shifted to the lower first molars. Without the banding of any other teeth or the employment of arches, the results shown were obtained. This case continued to

Fig. 3 Photographs of three faces showing marked deviation of facial harmony. Here the genes seem to have played havoc.

Fig. 4 Photographs of patient before treatment at nine years of age (top); at the end of treatment at twelve years (middle) and two years after completion (bottom).

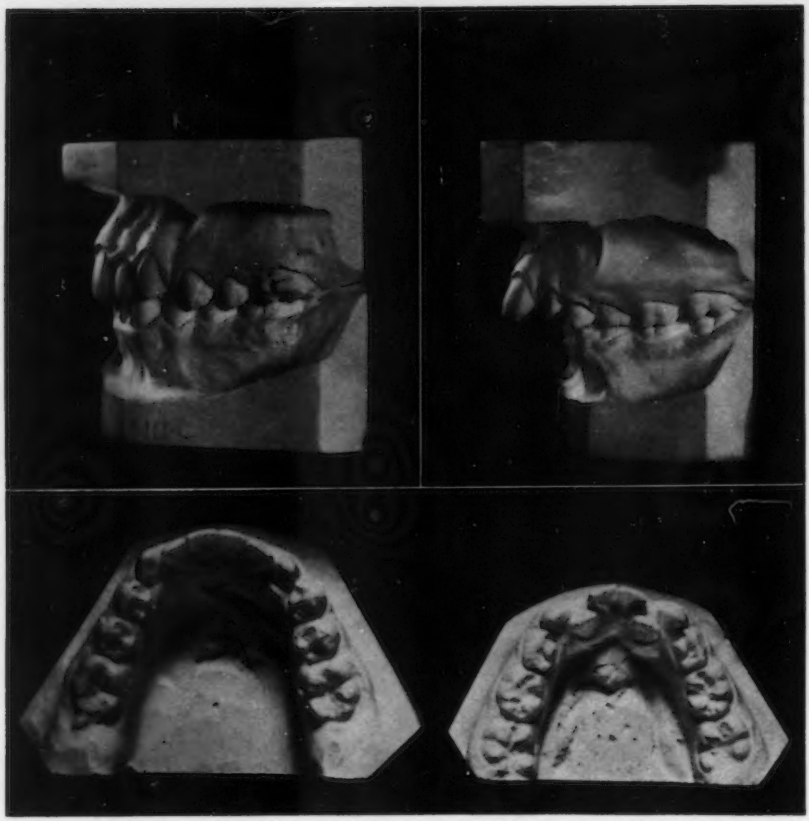

Fig. 5 Models of patient shown in Fig. 4. Before treatment, right; two years after completion, left.

improve after treatment.

The case shown in Figures 6 and 7 is quite similar to the previous one so far as the crowded mandibular incisors are concerned, although the facial type is different. With the same method of treatment as that described above, the result shown (Fig. 6) was obtained in a period of two and one half years after the eruption of the complete permanent dentition. I wished to place a complete appliance for the finishing of details but the patient refused. The final records show the case at age thirteen. This patient persists in a lip-biting habit which, I believe, accounts for the slipping seen in the lower anterior region.

In contrast to these last two cases, note the records of the girl illustrated in Figures 8 and 9. She was thirteen years old when treatment was started with a complete appliance following the extraction of the four first premolars. Although a satisfactory occlusion was obtained, there was no improvement in the face. Early, simple treatment might well have obviated the

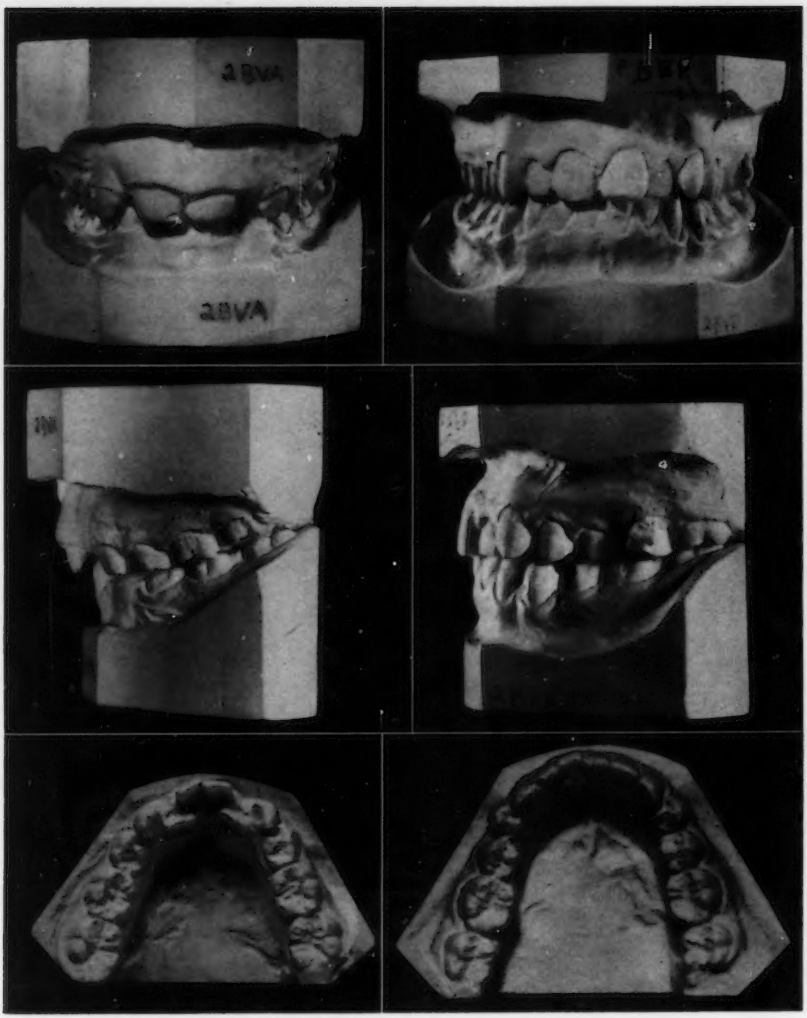

Fig. 6 Models of patient shown in Fig. 7 before treatment (left) and a year after completion (right).

Fig. 7 Photographs of patient before treatment at eight and one half years and at thirteen years, after treatment with bite plane and cervical traction had been discontinued for over a year.

necessity for extraction but would not have improved the appearance.

The complexity of our problem makes it absurd to expect to treat eighty or ninety per cent of our cases by formula. No philosophy can obtrude the truths of biology by proclaiming the face of

Fig. 8 Photographs of patient after treatment which included extraction of four first bicuspids.

Yoknapatawpha County as the true creation, and the contracted, diminished arch — the true archetype. Yet we constantly deceive ourselves that such pat theories and philosophies are indeed the truth and that happy formulae may eventually remove the onerous task of thinking for ourselves. Such theories attract followers who cling to the precepts taught them long after they have been completely disproved.

I have treated six members of one family, all of whom had Class II, Div. I malocclusions with long, contracted faces and narrow arches. Both parents had the same type of malocclusion. Of the six cases, three remained stable in ideal occlusion over the years. One was stable for over five years, when a lower lateral incisor began to slip lingually. It had to be extracted ultimately because it became an irritant. The space closed without help by itself, and the rest of the dentition is now well balanced.

Another of the children (Fig. 10) suffered total relapse after two or three years, and the upper second molars were extracted at seventeen years of age. Occipital traction of one year's duration completed the case, and the third molars erupted in the second molar positions. The sixth case also relapsed and was retreated by the removal of the impacted third molars at sixteen years of age followed by the employment of cervical anchorage. If I had been able to choose the time, all of these children would have been treated by means of extraoral anchorage in the deciduous dentition.

Of relatively recent date we have witnessed what is to my mind one of the greatest misuses of a valuable research tool ever to be placed in the hands of the orthodontist. I refer to the cephalometric x-ray developed by Broadbent for the scientific study of the

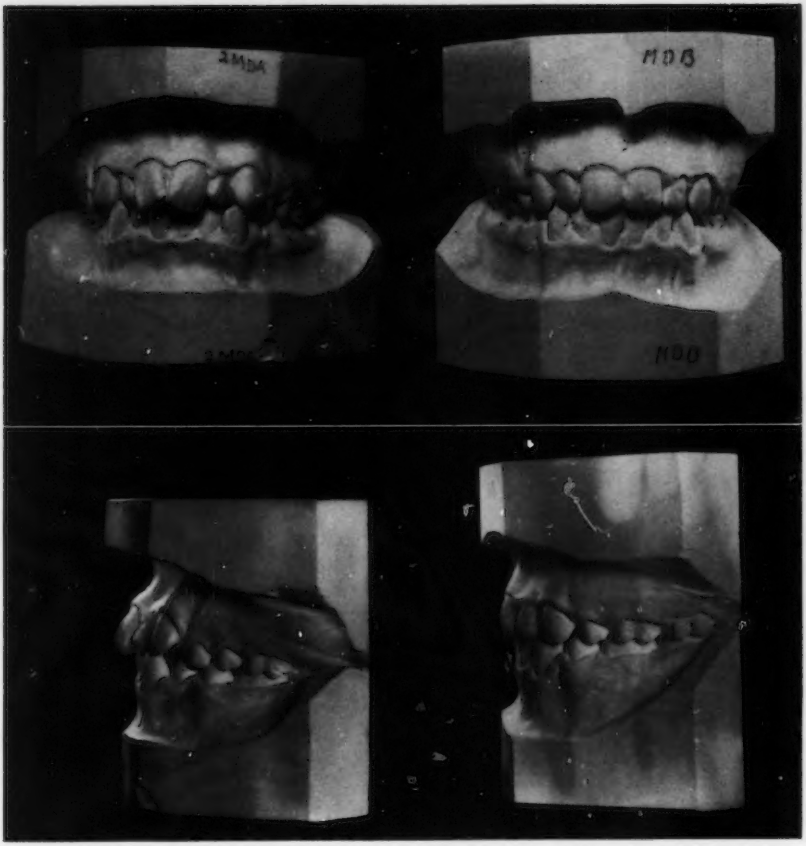

Fig. 9 Models of case shown in Fig. 8 before (left) and after (right) treatment by extraction of four bicuspids.

growth of the head. It made possible the study of the changes that look place in relation to *time*. But this most distinctive advantage of the method is the one that is ignored.

It has become routine practice in many offices to trace the lateral headplate, measure certain angles and dimensions, throw them into the statistical grab bag, shake well and pick out the answer. This answer seems, in a suspiciously large percentage of cases, to indicate an insufficiency of development for the accommodation of all of the teeth. It never seems to be realized that this headplate tracing — made from a picture snapped at one moment in time — represents the individual at only that one instant — it tells nothing of what he has been or what he will become — only his present morphology, but nothing of his physiology or fate.

And what many cases do become is a constant source of wonderment to one who has the patience modestly to aid Nature as the child grows. Some-

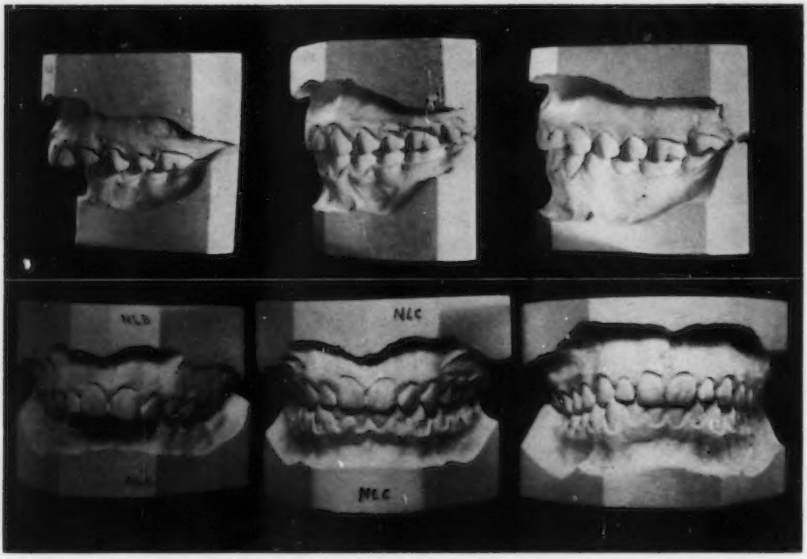

Fig. 10 Case before treatment shown on left; at beginning of relapse in center, and after retreatment by extraction of upper second molars and cervical traction of one year's duration.

times the much deplored bimaxillary protrusion disappears or seems to fade into pleasing proportionality with the face, if left alone.

I sometimes doubt the advisability of a critical appraisal of the headplate before treatment is completed. The analyses, both angular and linear, keep getting more and more complicated as more Ph.D's are conferred, and each seems to set further limitations on us. As we learn of the problem of the infinite number of integrations of parts that must prevail in order to bring forth a "normal" face, it seems that we can hope for little in the way of complete success.

The child, far from being a unit fixed at one stage, is shown to be an ever-changing organism, the parts of which are constantly growing at rates and at times that refuse to obey statistical laws. How these may ultimately make out in any given case will determine whether harmony of function and form will ultimately prevail. But it should be recalled constantly that the child is the result of millions of years of human genetic influences, and that during growth he is retreading that path. This, of course, explains why the results of treatment by competent men so frequently belie the pessimism of the prognosis. It is surprising and not very flattering to us that we are taken in so easily by scientific jargon. A wise man once said, "if your clinical observations do not agree with your laboratory findings, you had better throw away the laboratory findings."

The influences to which I have just referred might all be lumped under the heading of "heredity". They are the things over which we have little, if any, control. But there are other influences than these which act on the

child's face as he goes along, and it is sometimes possible to do something about them. You will at once think of habits and the word "habit" could correctly be applied, although I am not thinking here of these very evident acts, such as the sucking of the thumb or the wetting of the lips. Rather I have in mind those subtle states of muscular posture or function that endow each one of us with a distinct personality. These muscles of our face are recent acquisitions, still very unstable although very adaptable. They respond to all sorts of states and conditions and frequently persist in an adaptive act long after the need for it has passed. The child with an abnormal jaw relationship such as Class II cannot use his musculature according to the accepted pattern; he must adapt to what he has. It would seem wise, therefore, to establish normal relations for him at an early age so that his adaptation to the normal be possible.

Beyond these rather easily understood conditions there is another group which would have to be called psychosomatic. Our rising rates in heart disease and duodenal ulcers are not the only indices of the increasing stresses of modern life. The faces of American children, when contrasted with those of more leisurely lands, tell much to the observant eye. What the orthodontist can do here, I do not know, but I am convinced that more than one broken lower incisal contact could be blamed on such causes. Every nuance of muscle movement, conscious or unconscious, every acquisitive reflex responding to internal or external stimuli, is relayed to the facial musculature where idiosyncrasy of patterns of activity, once set up, brings on consequences that may affect ultimately the equilibrium and stability of some segment of the denture.

If we are going to wait to see what the child will look like as a young adult, or if we do not have the opportunity to see him before that time, it seems indeed that we are limited in what we can do. Under these circumstances, compromise may be indicated. But it seems presumptuous, to the author at least, to set ourselves up as the judges of the outcome of such a little-understood process as that of growth. There is more than a little truth in the adage that a homely baby makes a beautiful adult and orthodontists and pediatricians should know this above all other men. On the other hand the extraction of teeth for aesthetic reasons or for ease of handling in many cases seems to lead to only fleeting improvement in many cases and is soon lost.

Figure 11 presents the photographs of a boy of seven years with a very disfiguring Class II malocclusion who was treated to a Class I occlusion without much improvement of his appearance.

Fig. 11 Photographs of patient of seven years of age before treatment (above). Below are photographs of same case after second period of treatment which included extraction of four first bicuspids.

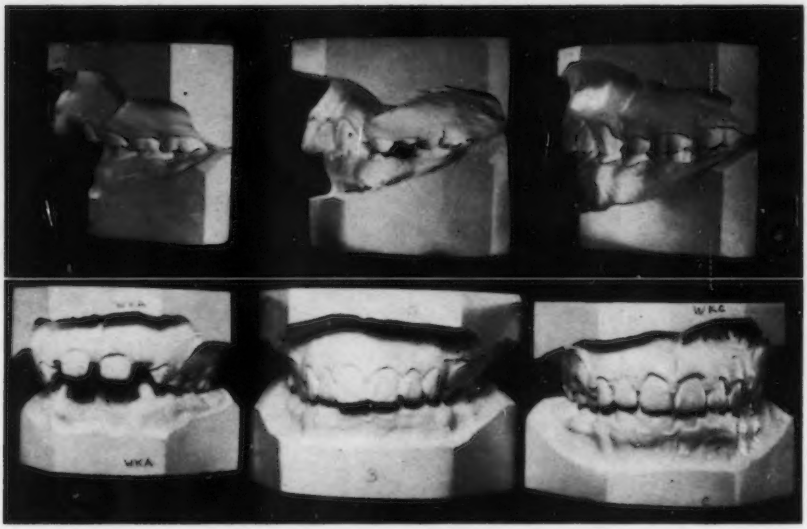

Fig. 12 Models of case shown in Fig. 11 (left) before treatment, (center) after reduction of Class II relation, and (right) after extraction of four first bicuspids and full appliance treatment.

Upon the complete eruption of his second dentition at about twelve years of age, there having still been little improvement in the fullness of the mouth, it was decided to extract the four first bicuspids and close the resulting spaces.

The models of the case at the beginning of treatment, at the time the bicuspids were extracted and following the completion of treatment are shown in Figure 12. I seriously doubt that the end result in this case was affected significantly by the extractions.

The photographs of the patient shown in Figure 13 before treatment, following the extraction of four bicuspids and ten years later tell the same story. One is reminded of those early cleft palate, harelip surgical procedures by which hideous monstrosities were changed to cute babies, only to slip gradually back into lifelong ugliness.

Treatment in the Primary Dentition

No one would deny that good occlusion in the first dentition with the jaws in a Class I relation and the face well developed is a favorable precursor to the same desirable conditions in the permanent dentition. It is also true that some temporary disharmonies of the early mixed dentition are self-correcting or correctable by very simple means, if properly timed.

The Class II and Class III malocclusions, however, do not correct themselves, and it is these cases that give parents and pediatricians concern. They cannot understand why, if other deformities are more easily corrected at an early age, the same should not hold true for the face. The early correction of the occlusion is the greatest aid to the promotion of the health of the denture through the development

Fig. 13 Photographs of patient (top) at beginning of treatment, (middle) after extraction of four bicuspids and full treatment, (bottom) ten years later.

of normal masticatory and facial habits as well as the beauty of the face.

The claim could not be made that the treatment of the three year old is as pleasant as that of the child of eight years, and it is certainly not recommended as a pastime to the orthodontist short on patience. It is difficult, time consuming, but rewarding. Many thus treated never need further attention, and even those who do show some relapse seem to be easily corrected later. Rapidity of result depends on the severity of the condition to some extent, but more on the state of health and rate of growth of the child.

Figure 14 represents a very severe case which was first treated by simple extraoral means at three and one half years of age for the correction of the jaw relation. The lower arch was in complete lingual relation to the upper, and it was necessary to band the buccal teeth and employ bucco-lingual intermaxillary elastics in conjunction with

Fig. 14 Photographs of boy three and one half years at start of treatment (top), after six months of treatment with simple applianee (middle), and fourteen years after finishing treatment with appliances (bottom).

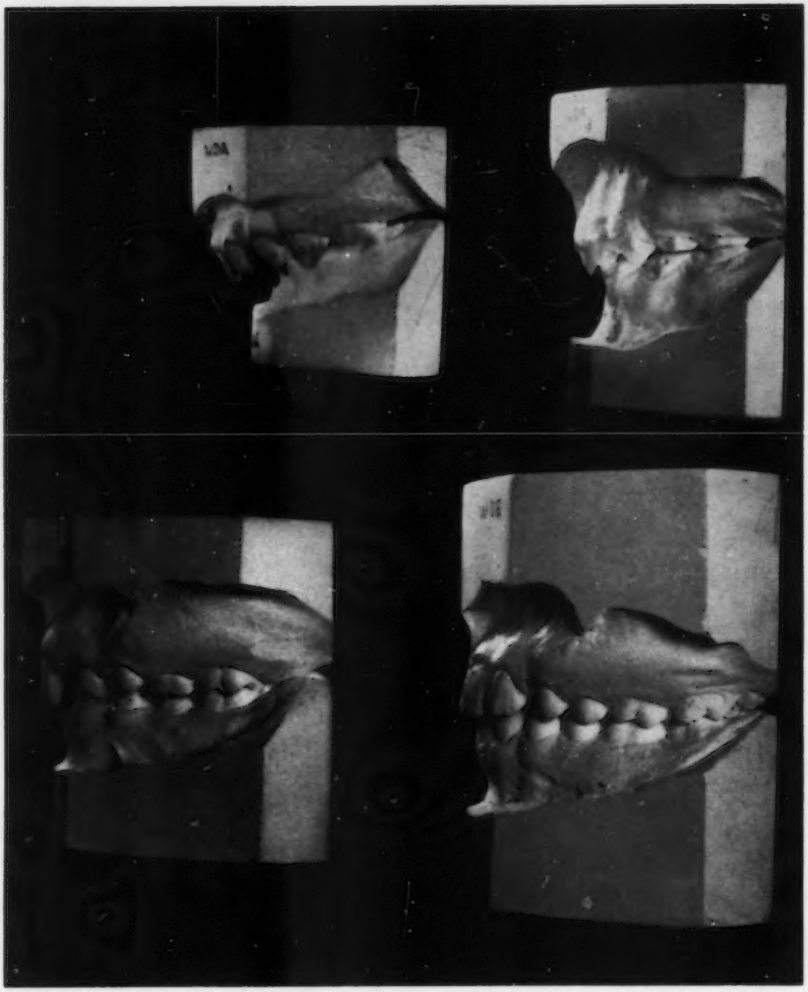

Fig.15 Models of case shown in Fig. 14. Top models before and after six months of simple treatment described in text. Bottom, models before and after appliance therapy to position upper left canine.

the face-bow and cervical traction. Upon the eruption of the permanent teeth, the case was completed by a short period of full appliance therapy to position the upper left canine in the place of the missing lateral incisor. Models of this case are shown in Figure 15.

As in all orthodontic treatment, we must be prepared to encounter variations in patients' response to treatment. In some there will be correction of the entire arch without change of its form; in others the buccal segments will be corrected, but the anterior teeth will remain nearly in their original positions. Such cases are frequently benefited by a retainer of the Hawley type, the incisors being retracted by periodic tensing of the labial wire. Too much attention should not be paid to the detailed placing of individual teeth before the denture is fairly complete, allowing natural forces to do what they will unhindered. The results are frequently surprising. And early treatment, followed by rest, permits a molding of the denture by its own peculiar and individual forces that lends a naturalness to the result quite at variance to many of the artificial looking dentitions created by later treatment.

Needless to say, all successful treatment in orthodontics depends on the growth of the child, and it would be of inestimable value to the orthodontist to know when the jaws are going to exhibit their maximum growth. We do not have this knowledge at present and must rely on the generalization that the tempo of growth diminishes with age. The only conclusion this permits us is that, the earlier treatment can be instituted, the better one's chances for success.

750 Main Street, Hartford, Conn.

Clinical Observations Of Cases Five Years Out Of Treatment

HAROLD M. STACKLER, D.D.S., M.S.D.

Chicago, Illinois

This paper treats of observations made on a small sample of twenty Class II, Division I extraction cases which have been out of treatment a minimum of five years. These are all four first bicuspid extraction cases except two, one of which involves a second bicuspid and three first bicuspids, and the other, two lower first molars and two upper first bicuspids. By "out of treatment" I mean that the time was determined from the date the appliance was removed, and thus includes retention periods of varying lengths.

The purpose of this study was to determine clinically what changes had taken place during the five year interval. More specifically, I sought answers to the following questions:

1. What happens to the spaces that are not closed at the extraction site?
2. Do spaces develop at the extraction site after treatment?
3. Is there a tendency for deep overbites to occur?
4. What is the degree of overlapping of the mandibular incisors?
5. Do the mandibular incisors maintain themselves in their treated positions in reference to labial-axial inclinations?
6. Does the esthetics improve as the patient matures?

Regarding spaces that remain at the extraction site when the appliance is removed, or spaces that develop soon after the removal of the appliances, the evidence overwhelmingly demonstrates that these spaces tend to close. However, they generally do not close in a favorable manner; that is, although the cuspid and second bicuspids may reach a contact relationship, the second bicuspid does not maintain a vertical axial inclination, but, rather, a definite mesial axial inclination due to a mesial tipping into the space, so that the mesial marginal ridge of the second bicuspid contacts the cuspid well below the normal contact area. In addition, there may be a definite mesial tipping of the first and second molars. This situation was more frequently noticed in the mandibular denture and presents a sort of two-level line of occlusion, the posterior segment being on a lower level than the anterior segment (Fig. 1).

One case (Fig. 2) may be demonstrated in which this phenomenon occurred with no cuspid—second bicuspid spacing. Upon completion of treatment, the cuspid and second bicuspid were in satisfactory vertical inclinations and in a good contact relation. Five years later, however, there is a noticeable mesial tipping of the mandibular buccal segments with a ducking of the second bicuspid below the cuspid contact area.

There is no evidence that spaces developed at the extraction site at this time interval. However, as mentioned previously, spacing at the extraction site may occur soon after the appliances have been removed. These spaces tend to close in the manner just described.

Presented before the Midwestern Component of the Angle Society, Chicago, January 1957.

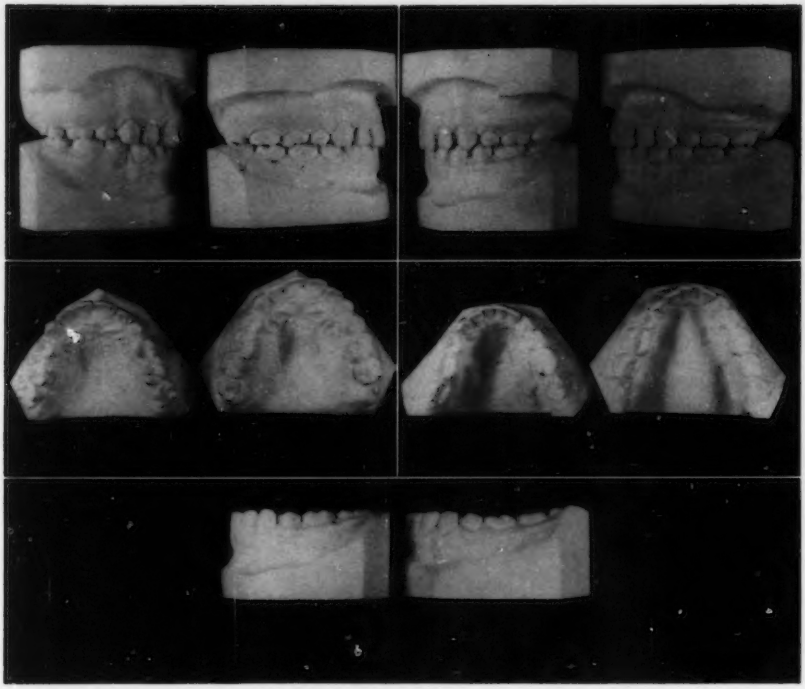

Fig. 1 Top: Left and right sides, soon after treatment and five years later.
Middle: Occlusal views.
Bottom: Lower left sides.

Three of the cases observed show a loss of vertical dimension, or an overbite that may be considered excessive. In each case, the severity was not as great as initially, but excessive enough to arouse concern. This loss of vertical dimension is thought to be due to a combination of two factors: 1. an elongation of mechanically depressed maxillary incisors, and 2. mesial tipping of leveled buccal teeth (Fig. 3).

Broken contacts or overlapping of

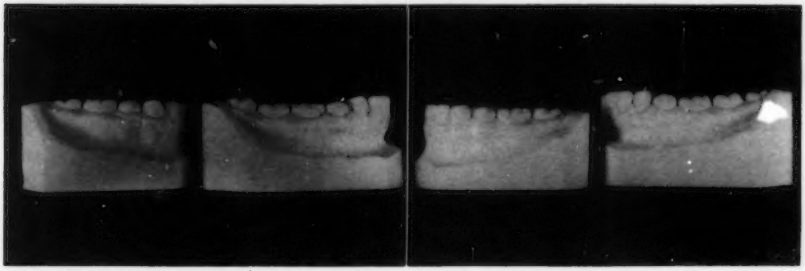

Fig. 2 Right and left mandibular views, soon after treatment and five years later.

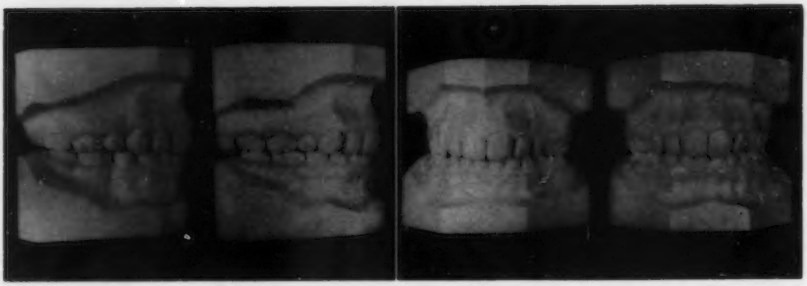

Fig. 3 Right lateral and anterior views, soon after treatment (left) and five years later (right).

the mandibular incisors occurred in sixteen of the twenty cases observed. This overlapping varied from a slight rotation of one incisor to a definite blocking out of an incisor. The most severe case of overlapping mandibular incisors may be seen in Figure 4. This observation tends to indicate that some degree of overlapping occurs in a large percentage of the cases.

Clinically, it is somewhat difficult to determine whether the mandibular incisors tend to maintain themselves in their treated positions or whether there is a tendency for them to tip forward again. However, there are nine cases, almost half of the sample, where it appears as though there has been some forward tipping. Slight as this tipping may be, it seems to be reflected in the face, causing a slightly more protruded lip than when the active treatment was completed. Less I be misunderstood, may I emphasize that the amount of forward tipping of the mandibular incisors and the amount of protrusion of the lower lip that may reoccur are nowhere near as great as that present initially.

Facially, fifteen of the twenty cases observed appear better balanced and more harmonious at this time than at the time of completion of active treatment. This improvement is not due to any further reduction in the amount of protrusion of the lip area, but, rather, to the development of a button on the chin which seems to place the chin in a more forward position, plus, in some cases, a slightly more protruded lower lip than that at completion of treatment (Fig. 6).

Four of the twenty cases observed are about the same facially. This is primarily due to the failure of the chin to be in a more forward position. Two of the twenty cases seem to be worse facially: one due to a more protrusive lower lip and the failure of the chin to be in a more forward position, and the other, the development of a large button presenting a somewhat dished-in appearance.

Summarizing, 1. Spaces that remain at the extraction site after treatment, or spaces that develop soon after appliance removal, tend to close, but, gen-

Fig. 4 Occlusal views five years after treatment.

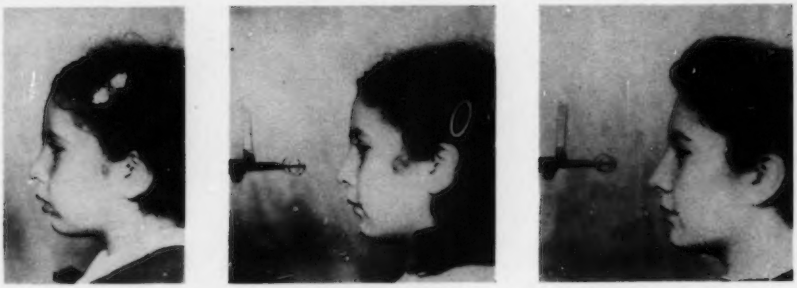

Fig. 5 Left, before treatment; center, soon after treatment; right, five years after treatment.

erally, the posterior teeth are tipped mesially into the space so that the mesial marginal ridge of the second bicuspid contacts the cuspid below the normal contact area.

2. There is no evidence of space development at the extraction site at this time interval.

3. Deep overbites occurred in this sample, but the evidence did not appear to be conclusive.

4. The mandibular incisors generally did not tend to maintain themselves in their treated positions with reference to labial axial inclinations, but, rather, there is a tendency for them to tip slightly forward. This action could suggest an attempt by nature to develop an occlusion which is in harmony with the facial skeleton which supports it, with the muscles of expression and mastication, and with the temporomandibular joint.

5. This study indicates that the facial esthetics tends to improve, and that this improvement is due mainly to the chin being in a more forward position.

7200 Exchange Avenue

The Angle Orthodontist

A magazine established by the co-workers of Edward H. Angle, in his memory . . .

Editor: Arthur B. Lewis.

Business Manager: Silas J. Kloehn.

Associate Editors: Allan G. Brodie, Morse R. Newcomb, Harold J. Noyes, Robert H. W. Strang, Wendell L. Wylie.

Vol. XXVIII, No. 2 April, 1958

The Angle Orthodontist

Instructions to Contributors

The author should submit the original typed copy of the manuscript, double-spaced on bond paper. The author's name and city of residence should appear immediately after the title, and the street address should appear at the end of the article.

Do not use symbols to indicate teeth; in *tables* teeth may be designated as U-1 or L-6, but in the text names of teeth should be fully written out.

Line drawings should be prepared in India ink on white stock; the width of lines and the size of lettering should take into account the fact that most illustrations are reduced in size in the engraving process. Photographs should be sharp, glossy prints of adequate size, *unmounted and kept separate from the text*. At the discretion of the editor, illustrations will be reproduced either as single column cuts of approximately 2¼ inches in width, or a double column cuts of 4¾ inches in width. The height of the cut may vary, but contributors should be keep in mind that proportionate reduction or enlargement of illustrations affects all dimensions, and that arrangement of material should be such that awkward proportions are avoided and no loss of clarity results.

The Frankfort plane should appear in cephalometric tracings, and wherever possible should provide the basis of orientation of illustrative matter portraying the face, whether it be films, tracings or facial photographs.

Extraneous details should be eliminated from illustrative matter in order to keep the salient portions of the cut as large as possible, i.e., the cranial outline should be eliminated from cephalometric tracings unless its ommission would be detrimental to the sense of the article, photographs of patients should be cropped closely, and photographs of models should be separated by a minimal amount of empty space. Illustrations will be returned to authors for revision when they do not comply with these specifications.

Editor
Arthur B. Lewis
804 Hulman Bldg.
Dayton 2, Ohio

Business Manager
Silas J. Kloehn
Zuelke Building
Appleton, Wisconsin